Three Essays on Firm Productivity

by

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Thesis

Submitted to the University of Warwick

for the degree of

Doctor of Philosophy

Department of Economics

April 2018

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Acknowledgments

I express my sincere gratitude to my supervisors Christopher Woodruff and Rocco Macchiavello for their continuous support and guidance for the entire duration of my PhD. I would also like to thank the faculty members at the Department of Economics of the University of Warwick, for their helpful comments and suggestions. This thesis would not have been possible without the encouragement and enthusiasm of my parents, Zahurul Haque and Akhter Jahan Khanam. I am deeply indebted to them for this and countless other things. I am thankful to my other family members and relatives as well, who have also supported me in numerous ways during my PhD.

Declarations

All the chapters in this thesis contain original research work. They have not been submitted by me for any other assessments or previous degree courses. Chapter 1 is solely my own work, which incorporates comments and suggestions from my thesis supervisors, Christopher Woodruff and Rocco Macchiavello, as well as from others I have discussed this work with. Chapter 2 is jointly co-authored with my supervisors, as well as Robert Akerlof and Atonu Rabbani. We jointly planned and conducted the required field work, and produced the required analysis. Chapter 3 is jointly co-authored with my supervisors and Atonu Rabbani. We used data collected through previous projects of my supervisors, but worked together to analyse the data.

Abstract

This thesis consists of three chapters. Chapter 1 aims to understand how performance-based ranking affect productivity of workers. While providing such ranking may induce workers to increase effort because of status concerns, such information may also demotivate them or make them wary of outperforming peers. This chapter disentangles the effects of demotivation, social conformity, and status associated with ranking. I implement a randomized experiment at a Bangladeshi sweater factory that pays employees on piece rates. Treated workers receive monthly information on their relative performance either in private or in public. A simple theoretical framework shows that intrinsic status concerns induce Private Treatment workers to increase or decrease effort depending on the feedback they receive from the intervention. Workers in Public Treatment respond similarly but face two additional incentives - social status (positive effect) and social conformity (negative effect). Empirical evidence shows that Private Treatment workers increased (decreased) effort upon receiving positive (negative) feedback. Public ranking led to lower net effort relative to Private Treatment because of a strong preference not to outperform friends. The negative effects from demotivation and social conformity may explain why the existing literature finds mixed evidence of impact of ranking workers.

In Chapter 2, we look at how firing of workers in an organization affect the productivity of the surviving co-workers. We take advantage of detailed individual-level production records from, and extensive fieldwork conducted at, a large Bangladeshi sweater factory before, during, and after several episodes of labour unrest that eventually led the management to fire approximately 25 percent of the labour force on the relevant production floor. Exploiting across-worker variation in exposure to colleagues' terminations, we document a negative impact of the firings on productivity of surviving workers. Fired co-workers' spatial proximity drives the results. Additional evidence rules out a number of competing mechanisms such as subsequent targeted punishments from management, loss of productive peers, or attention diverted to help recently hired and inexperienced co-workers. We argue that the effects are likely driven by workers' feelings of loss or anger towards the management.

Chapter 3 studies the relationship between external shocks, such as political strikes and labour unrest, and productivity in the ready-made garment sector in Bangladesh. Using data from 33 ready-made garment factories in Bangladesh and adopting an event-study approach, we document very little change in productivity or worker absenteeism during political strikes lasting two days or less. Productivity falls when strikes last five days or more. The main channel for such fall appears to be supply-chain disruptions rather than worker absenteeism. However, absenteeism and quality defect rates increase immediately during labour unrest, resulting in a decrease in output. As a benchmark comparison, we show that the drop in productivity from sustained strikes or labour unrest is equivalent to a fall in productivity due to an increase of about 7 degrees centigrade in temperature.

Chapter 1

Do Performance Ranks Increase Productivity? Evidence from a Field Experiment[1]

1.1 Introduction

A growing literature on status concerns suggests that firms can increase the productivity of their workers by providing them with performance-based, relative rankings. Both theoretical research (e.g., Besley and Ghatak [2008]) and empirical evidence (e.g., Blanes-I-Vidal and Nossol [2011]; Ashraf et al. [2014a]) suggest that firms benefit from such status incentives. But if gains are to be had by introducing ranking at the workplace, why don't more firms do it? Is there more to ranking than the simple, positive effect of status concerns? Perhaps yes; some evidence indeed suggests negative average effects from rankings (e.g., Barankay [2011]; Blader et al. [2014]). However, less understood is the key issue of why telling workers their relative positions may induce them to lower their efforts.

[1] This research was financially supported by Private Enterprise Development in Low-Income Countries (PEDL). The experiment for this paper had been registered with AEA RCT Registry on December 17, 2015 with ID-0000981. Shusmoy Roy and A. Latif Patwary provided excellent research assistance. I am grateful to Christopher Woodruff and Rocco Macchiavello for their continuous support, and to Robert Akerlof, Roland Rathelot, James Fenske, Alexandre Mas, Jordi Blanes-I-Vidal, Emma Duchini, Arun Advani, Nicholas Bloom, Oriana Bandiera, Nava Ashraf, Giorgio Zanarone, Florian Englmaier, Joachim Winter, Kristina Czura, Simeon Schudy, and seminar/conference participants at University of Warwick, Paris School of Economics, University of Oxford, NEUDC 2017, CUNEF, University of Surrey, and LMU for helpful comments.

In this paper, I argue that there are at least two reasons that such rankings may lead to reduced worker effort: One derives from a worker's *intrinsic* motivation for status. A worker who receives information about his relative position may either be motivated or demotivated, depending on whether he previously believed his position to be higher or lower than shown by actual rankings.[2] Thus, this discrepancy can lead to either an increase or a decrease in his effort. Second, in situations that make ranks public, workers may also be subject to *social* concerns. As the result of being known to others, higher rankings potentially generate higher social status. At the same time, however, a worker who increases his rank imposes a negative externality on the others whom he outperforms. So, the worker may internalize this externality, and may reduce effort to avoid being seen as a self-serving person, and to avoid risking being socially ostracized by co-workers, particularly those with whom he has close interaction.[3]

This paper aims to disentangle the positive effects of status concerns from the negative effects of demotivation and social conformity - all of which may affect the productivity of workers when they are ranked by a firm.[4] Disentangling these effects poses empirical challenges. First, clear measures of individual performance must be available. Second, a distinction must be made between intrinsic and social incentives. Third, to understand potential motivational and demotivational effects, workers' prior perceptions about how they rank in comparison with their co-workers must be known. And finally, to test social conformity, a worker's reference group, the network of people with whom he may seek to conform, must be identified.

I overcome these challenges by combining a novel experimental design with detailed pre-intervention data on workers' self-perceived ranks, and on workers' social networks. Working with a leading sweater factory in Bangladesh, I provide workers with their performance-based ranks. I work with a specific section in the factory that employs 366 workers, all of whom receive payment based on individual production. During the 10-month-long intervention, control-group workers received monthly summary information about their production in the previous month. Treated workers received this same information, and, in addition, they were told

[2]Recent evidence has indeed shown that workers can become demotivated from relative concerns (Breza et al. [2018]).

[3]Indeed, the theoretical literature on conformity (e.g., Akerlof [1997]; Bernheim [1994]) suggests that workers may not want to deviate too much away from their peers, lest they face social punishment.

[4]I use the term *conformity* in a slightly weaker sense than is traditionally used in the literature. In the literature, the term *conformity* refers to people's urge to converge to a single point, whether from below or above. In the context of this paper, however, converging to a rank from below is observationally equivalent to pursuing status incentives, and hence, not empirically identifiable. Hence, conformity can be observed only when it is convergence from above.

their relative ranks. There were two treatment groups; workers in a given treatment group were ranked among co-workers in the same treatment group. In the first group, *Private Treatment*, workers were told only their own ranks. In the second, *Public Treatment*, all the workers were told all rankings - both their own and those of other workers in the Public Treatment. The two treatments allow me to separate the effects of intrinsic and social incentives. A baseline survey conducted prior to the intervention recorded workers' own beliefs about what they expected their position to be in the ranks, and provided a detailed map of their social network. The information on the workers' expected ranks allows me to determine whether the information on their true position provided a positive or negative surprise (feedback) to the worker; thus, this allows me to subsequently identify motivational and demotivational effects. The social network map allows me to disentangle social concerns into social-status and social-conformity components.

I provide a simple theoretical framework to interpret the empirical design and results. There are two key insights from the theoretical framework: First, how a worker responds to the intervention because of intrinsic-status concerns depends on the shape of the underlying intrinsic-status utility curve. If the status utility from rank is convex, a positive feedback will motivate him to increase his effort. This happens as the worker realizes that true marginal utility (now that he is at a higher rank than he had expected) far outweighs the marginal cost of his effort. On the other hand, a worker who receives a negative feedback will be demotivated and decrease his effort. The predictions will be opposite if the status utility from rank is concave. These predictions can be tested empirically to determine the shape of the underlying status-utility curve. Second, while a Public Treatment worker responds to intrinsic status concerns in the same way that a Private Treatment worker does, a Public Treatment worker also responds to social incentives. He faces two additional incentives, social status and social conformity. Relative to a privately ranked worker, social-status incentive will induce the worker to increase effort in order to achieve a higher rank, but pressure to conform to peers may pull his effort down instead. Hence, relative to a privately ranked worker, a publicly ranked worker will exert more effort as long as he is ranked below the peers who can socially punish him. If he is ranked above them, he will exert relatively less (more) effort if the marginal disutility from outperforming peers is higher (lower) than the marginal social-status utility.

There are two key empirical findings: First, the response of workers to the private treatment depends on their prior beliefs about their relative positions, with those actually ranked higher (lower) than their perceived ranks increasing (decreasing) productivity. This suggests that, for these workers, the marginal return to status is increasing with rank (status utility curve is convex). Workers who received positive feedback in the first month of treatment performed more than 2.5 percentage points (p.p.) better than control-group workers who would have received positive feedback had they been ranked. Workers who received negative feedback, however, performed about 4 p.p. worse than those who received positive feedback, and more than 1 p.p. worse than those in the control group. The gain in productivity from one group was offset by the loss in another, as a larger share of workers received negative feedback ('a negative surprise'). Hence, the average treatment effect was positive but statistically insignificant.

Second, making ranks public led to worse outcomes than in making them known in private when workers were ranked higher than their friends. Workers in the Public Treatment group who ranked higher than their *friends* (defined as workers with whom they had social interaction outside the factory, as reported at baseline) reduced their performance by more than 3 p.p. on average compared to those in the Private Treatment group. This conformity occurred only with respect to friends and not with respect to any other peer group, which is consistent with the hypothesis that workers conform out of fear of social punishment. Once the response to social-conformity incentives is accounted for, social status shows a small, positive, but statistically insignificant effect. As a result, average effect of Public Treatment was weakly worse than that of Private Treatment.

I also provide additional findings that support the interpretation of the results. While negative feedback in the first treatment month had an overall negative effect on workers, not all workers gave up and reduced effort. Conditional on receiving negative feedback in the first month, workers showing more competitive attitudes in a baseline laboratory-in-field experiment performed better after the intervention. While non-competitive workers reduced their effort by about 3 p.p. in response to negative feedback, competitive workers performed about 4 p.p. better than them. This was true for both treatment groups. This serves as additional evidence that workers cared about their ranks; it also underlines how the same private-ranking treatment elicited opposite responses from different groups of workers.

The subject of providing feedback to workers about their relative ranks has attracted attention across a wide range of fields within economics, including management (Blanes-I-Vidal and Nossol [2011]; Kuhnen and Tymula [2012]), education (Azmat and Iriberri [2010]), and public policy (Ager et al. [2017]; Chetty et al. [2015]).[5] Nevertheless, the results from this literature, especially that on firms, are

[5]See Kluger and DeNisi [1996] for a discussion of findings in the field of psychology.

conflicting and remain far from conclusive.

Specifically, with respect to the literature on firms, studies about private feedback on workers' relative ranking document a wide array of impacts. Blanes-I-Vidal and Nossol [2011] find positive impact; Blader et al. [2014] find zero impact; and Barankay [2011, 2012] finds negative impact. However, the source of such variation in impact across these papers is unclear. A possible clue lies in Breza et al. [2018]. From their experimental study, Breza et al. find that workers become demoralized, and reduce effort when they realize that they are paid relatively less than their peers. Can such a demoralization effect explain negative effect from ranks? Possibly yes, but a priori it is not clear. In the context of Breza et al., the demoralization effect stems from wage inequality. Because the wages are determined by the firm, a worker cannot affect this inequality. On the other hand, ranking provides a different context; instead of reducing his effort upon receiving a negative feedback, a worker may increase effort to try to achieve a higher relative position. A formal test of a demoralization effect from ranks has not been done in existing papers.[6]

Studies with public ranking also find conflicting evidence of impacts. Ashraf et al. [2014a] and Delfgaauw et al. [2013] find positive effects; Ashraf et al. [2014b] and Blader et al. [2014] find negative effects; and Bandiera et al. [2013] find no effect. Again, it remains unclear why the evidence is so mixed. The demoralization effect remains one possible explanation; however, comparing the contexts in the papers suggests a second possible mechanism. Delfgaauw et al. [2013] find a positive effect from sales competitions among retail chain stores in Netherlands. On the other hand, Bandiera et al. [2013] find zero effect from public ranking among workers at a fruit-picking farm who were living in the same quarters for a fair length of time. A closer inspection reveals that the context in Bandiera et al. [2013] is more conducive for deeper social ties and, hence, stronger incentives to internalize negative externalities than in Delfgaauw et al. [2013].[7] More direct clues lie in Blader et al. [2014]. The study, which involved of truck drivers in a U.S. transport company, took place when the company was in midst of a management intervention that encouraged teamwork and collective effort. In this context, Blader and his co-authors find both positive and negative effects of public ranks; positive effects came from sites where the management intervention had not yet taken place, while the negative effects

[6] Barankay [2011] does raise this issue in his working paper, but cannot provide definitive evidence for the lack of data on workers' prior beliefs about their ranks.

[7] Bandiera et al. [2005] use a similar context (pickers at a fruit farm in the UK), and indeed find that workers in this setting internalize negative externalities imposed through a relative pay scheme.

came from sites that had received the intervention. The authors speculate that the intervention may have reinforced social ties among drivers. To underscore, this is conjecture; the role of social network in ranking-based incentives has not been studied in existing literature.

This paper contributes to the literature and provides new understanding about the dynamics on ranking-based incentives by proposing demotivation and social conformity as two channels that can explain why existing empirical evidence is mixed. For instance, evidence on the demotivation effect found in this paper suggests that the average effect of revealing true ranking information may be positive or negative, depending on whether uninformed workers, on average, overestimate or underestimate their relative performance. Also, the evidence on social conformity suggests that such conformity can further negate positive effects from status motivation if rankings are made public. In the process of identifying the impact of these two channels, this paper also contributes by separating intrinsic and extrinsic incentives within public ranking. Using both private and public rankings in the same context, this paper identifies how much of the impact from public rankings is driven by intrinsic motivations. Except for Blader et al. [2014], existing studies with public ranks do not make this distinction in incentives.

The findings from this paper also add to a few strands of broader literature. As indicated earlier, the evidence of demotivation effect found in this paper relates to the recent empirical literature on how relative concerns demoralize workers (e.g., see Breza et al. [2018]; and Huet-Vaughn [2015]). However, this finding is also related to the theoretical literature on the role of self-esteem in economic decisions. For instance, Benabou and Tirole [2003] show how an agent uninformed of his own ability can get demotivated by an informed principal's actions that reveal the agent's true ability. The evidence on social conformity, on the other hand, relates to the literature on social incentives in presence of externalities within firms. While studies of the effect of positive externalities on productivity are more common (e.g., Mas and Moretti [2009]), those with negative externalities are relatively rare. One exception is Bandiera et al. [2005], who study a farm that pays workers through a relative pay scheme; that is, a worker's pay depends on the ratio of his own productivity to average productivity among all co-workers. In that context, higher effort by a worker implies lower income for all, and hence generates a negative externality. Bandiera et al. provide evidence that workers internalize this negative externality and withhold effort. This paper with ranking-based incentives provides further evidence on the effect of negative externalities. Additionally, this paper reveals that workers reduce effort even when such externalities are non-monetary in nature. Another stream of related literature is that on individuals' social-image concerns in more general settings. Evidence of such social-image concerns and conformity have been found in education (Bursztyn and Jensen [2015]) and in laboratory experiments (Bursztyn et al. [2016]). See Bursztyn and Jensen [2017] for a more detailed discussion on this literature.

In what follows, Section 1.2 describes the context and setup. Section 1.3 develops a brief theoretical framework that provides analytical predictions of treatment effects. Section 1.4 discusses the experimental design. Section 1.5 discusses the data, while Section 1.6 discusses the empirical strategy and the main results. In Section 1.7, I discuss alternative explanations to the findings in this paper. And finally, I conclude in Section 1.8.

1.2 Background

The experiment was conducted in partnership with a leading sweater factory in Bangladesh, and implemented in the Manual Knitting Section, one of three knitting sections in the factory. In this section, which is situated on one single floor, workers knit sweater parts using individually assigned manual knitting machines.[8] These sweater parts are stitched into complete sweaters in the next section, and eventually prepared for shipping in the subsequent steps of the production process. I focus only on the Manual Knitting Section because all the workers in this section produce similar output using almost identical capital input (yarn, manual knitting machine, etc.). Focusing on only one section allows me to measure and compare productivity cleanly across the workers.

Because the factory takes in multiple orders from multiple buyers at the same time, the Manual Knitting Section can be working on multiple styles (and sizes) of sweaters on a single day. Consequently, at a given point in time, different workers (*operators*, as they are called at the factory) can be working on sweaters of the same style and/or size, or different ones. These styles are assigned to them by *distributors*, based on the production plan. The operators are divided into 15 administrative groups called *blocks*, with each block supervised by one supervisor. The operators are paid based on piece rates and receive their wages at the end of a production month. The complexity of the sweater parts and the corresponding piece rates may vary across styles. A typical sweater contains various knitted components: a front panel, a back panel, and two sleeves. Usually, an operator is assigned to produce a

[8]The other two sections produce similar outputs but employ different technologies. So, productivities from these three sections are not directly comparable. One of the other two sections use semi-automatic machines, while the third employs fully automatic machines to knit sweaters.

batch of 12 complete sets of sweater panels. For a style of average complexity, the batch will take a worker around one day to complete.

Three attributes of this Section make it an appropriate setting for the empirical exercise in this paper:

Piece-rate pay. As mentioned already, the operators are paid piece rates; hence, each operator is responsible for his own production. The process of individual production makes it easy to measure individual productivity.

No promotion opportunities. Operators have no prospect of any kind of promotion. Operators can move up to the next level, to become supervisors; but because the average take-home wage of a good operator is usually higher than the supervisors' salaries, operators choose to be supervisors only when their productivity falls with age.[9] This rules out the possibility that any ranking intervention would induce the workers to rank well for extrinsic incentives such as promotion.

Workers with long tenures. Among the 366 operators working in this Section at the beginning of the experiment in January 2016, 236 were hired during the years 2004-2010, 16 over 2011-2012, and 114 in 2014.[10] Thus, most of these operators had been working at the factory for more than six years, which potentially helped them to form expectations of their own ranks, and also to form close social ties with their peers. Indeed, evidence of a strong sense of community among these workers was found in a recent companion paper (Akerlof et al. [2015]) that used data from the same factory to check whether the lay-off of peers had any impact on productivity of retained workers. Similar evidence was also found in a baseline survey conducted prior to the intervention in this paper.

1.3 Theoretical Framework

In this section I develop a simple theoretical framework to examine how a worker responds to ranking once such incentives are introduced by a firm. The framework constitutes of two stages. In the first stage, I assume that ranks generate only intrinsic status utility for a worker. This utility is intrinsic because it stems from the worker's intrinsic motivation to be good at whatever he does; there are no extrinsic incentives involved. Considering intrinsic status alone lets me explore how workers respond to true rank information in absence of extrinsic incentives. In the process, this also lets me identify the shape of underlying intrinsic status utility

[9]For instance, in January 2016, the average take-home pay of the 15 supervisors on the floor was less than that of a worker in the 33rd percentile.

[10]The factory hired another 95 workers during July-August 2016, after the intervention commenced.

curve. In the second stage, I introduce social concerns associated with ranks. Such concerns relate to how a worker wants to be perceived by his peers or other people around him (extrinsic).

To keep the theoretical framework simple, I consider only two periods. In the first period, there are no explicit information on ranks available at the workplace. In absence of such information, the worker has only a noisy signal of his relative performance. In the second period, the firm introduces ranking at the workplace and provides workers with their performance-based ranks.

1.3.1 Intrinsic Status Concerns

I start by first considering the case where there are no social concerns and workers are driven by only intrinsic-status concerns. There are two periods $t \in \{0, 1\}$; in $t = 0$ there are no explicit rank information available, while in $t = 1$ the firm introduces performance-based ranks. The ranks can be revealed either privately or publicly; it is inconsequential since the workers do not have any social concerns.

To be concrete, worker i in period t chooses effort e_{it} to maximize his utility $U_{it}(.)$ given by the following:

$$U_{it}(.) = W(\tilde{e}_{it}) - C(e_{it}, \alpha_i) + H\big(z_{it}(.)\big) \tag{1.1}$$

All the functions $W(.), C(.)$, and $H(.)$ are continuously differentiable at least twice. $W(\tilde{e}_{it})$ is utility gained from wage earned through effective effort $\tilde{e}_{it}$. Effective effort $\tilde{e}_{it} = e_{it} + \epsilon_{it}$ is the sum of effort exerted by worker e_{it} and an individual-specific and time-variant shock to effort, ϵ_{it}. The shock occurs after the worker chooses his effort, but he observes it once it realizes. This privately observed shock can be interpreted as task-specific characteristics or unanticipated instances that change the yield of effort e_{it}. ϵ_{it} is i.i.d, $\epsilon_{it} \sim g(\epsilon)$, where $g(\epsilon)$ is the PDF for $\epsilon \in (-\infty, \infty)$, and $E(\epsilon_{it}) = 0$. Utility from wage follows standard concavity assumption, i.e. $W_1(.) > 0$ and $W_{11}(.) \leq 0$.

$C(e_{it}, \alpha_i)$ is cost of effort e_{it} exerted by worker i with skill level $\alpha_i \in (0, \bar{\alpha}]$. Higher α implies higher skill. Cost of effort is convex, i.e. $C_1(.) > 0$ and $C_{11}(.) > 0$. In addition, $C_{12}(.) < 0$ for $e_{it} > 0$. That is, marginal cost of effort is lower for higher skilled workers at any positive level of effort. Also, $C_1(0) = 0$, i.e. marginal cost of effort is as low as zero at zero level of effort.

$H(.)$ represents intrinsic-status utility derived from the worker's *perceived* rank, $z_{it}(.)$, which is given by:

$$z_{it} = \left[\tilde{e}_{i,t} - \frac{1}{n}\sum_{j}\tilde{e}_{j,t}\right] + \delta_{it} \tag{1.2}$$

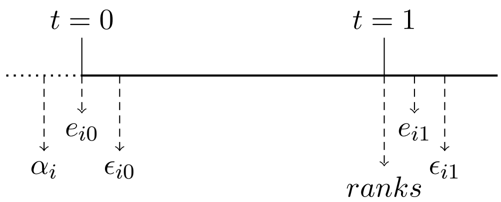
The expression inside the braces is worker's true rank, computed as the distance between his effective effort and the mean effective effort of the total workforce, where n is the total number of workers in the workforce. However, the worker only observes this rank with noise δ_{it}. $\delta_{i0} \in (-\infty, \infty)$ but $\delta_{i1} = 0$, for all workers, since they all find out about their true ranks in $t = 1$. Note that, $\delta_{i0} > 0$ implies that the worker overestimates his rank in period $t = 0$, while $\delta_{i0} < 0$ implies that he underestimates it; $\delta_{i0} = 0$ implies that he observes it perfectly.

Higher the perceived rank, higher is the utility from status; hence $H_1(.) > 0$. I do not impose any restrictions on the second order derivative of $H(.)$; as I will later show, the predictions of this framework will be determined by the curvature of $H(.)$. Also note that, the rank a worker i achieves increases with his own effective effort $\tilde{e}_{it}$, but decreases with that of others $\tilde{e}_{-i,t}$.

The functional form of $z_{it}(.)$ merits a little more discussion. Since I observe changes in only effort, I cannot separate the effect of marginal effort on $H(.)$ from that on the underlying rank function. So, I cannot identify the shapes of these two functions at the same time. Hence, without loss of generalization, a simplifying assumption made here is that the underlying rank function is linear in worker's own effort and that of others, and thus the effect of marginal effort on rank is constant. This lets me explore $H(.)$ alone. Nonetheless, the main intuition behind the results will be the same even with a more generic rank function.

However, a key assumption made in Equation 1.2 is that the noise in perceived rank of a worker is additively separable from his true rank. This lets the magnitude of distortion (bias) in perceived rank to be independent of a worker's original rank. On the contrary, if δ_{it} had entered z_{it} in a multiplicative form for instance, a fixed δ_{it} would introduce a higher magnitude of distortion at a higher rank than at a lower rank; this would have been a much stronger assumption to make.

The timeline of the events is given below:



Nature determines skill level α_i of worker i before the start of period $t = 0$. Next, worker i chooses his effort level e_{i0} at the beginning of $t = 0$. Soon after, effort to shock ϵ_{i0} realizes. At the very beginning of $t = 1$, the firm releases precise information on ranks of all the workers based on their performance in $t = 0$. This is unanticipated by the worker at $t = 0$. Subsequently, the worker observes his true rank and chooses his effort e_{i1}, following which, shock ϵ_{i1} realizes.

Because worker i does not observe shock to effort before determining how much effort to put in, he solves his optimization in expectation. In other words, he chooses e_{it} to maximize his expected utility. Using Leibneiz rule, and since $\frac{\partial \tilde{e}_{it}}{\partial e_{it}} = 1$, we get the following first order condition:

$$\int \Big[W_1(\tilde{e}_{it}) + \frac{n-1}{n} H_1(z_{it})\Big] g(\epsilon_{it}) d\epsilon_{it} - C_1(e_{it}, \alpha_i) = 0 \tag{1.3}$$

Assuming an interior solution, we have the following observation for a worker's response in $t = 1$.[11]

OBSERVATION 1: *If $H_{11}(.) > 0$, a worker who underestimates his rank in $t = 0$ ($\delta_{i0} < 0$) increases his effort in $t = 1$, while a worker who overestimates his rank ($\delta_{i0} > 0$) decreases his effort in $t = 1$. Conversely, if $H_{11}(.) < 0$, a worker who underestimates his rank in $t = 0$ ($\delta_{i0} < 0$) decreases his effort in $t = 1$, while a worker who overestimates his rank ($\delta_{i0} > 0$) increases his effort in $t = 1$.*

In other words, when intrinsic status utility from perceived rank is convex, a worker who has inaccurate information about his rank in $t = 0$ but receives positive feedback from information on his true rank increases his effort in $t = 1$. On the contrary, a worker decreases his effort if he receives negative feedback. Conversely, when intrinsic status utility from perceived rank is concave, a worker who has inaccurate information in $t = 0$ but receives positive feedback from information on his true rank decreases his effort in $t = 1$. On the contrary, a worker increases his effort if he receives negative feedback. The above observation is proved in the Appendix.

Intuitively, a worker increases his effort if information about his true rank reveals that his marginal status utility from an additional unit of effort is higher than he thought; he decreases effort if it is the converse. But whether it is the positive feedback or negative feedback that revises his marginal status utility upward depends on the shape of the underlying status-utility curve.

The above framework produces a key insight. If workers do not care about status from ranks (that is, $H(.)$ does not exist in their utility function), they should

[11] For an interior solution, I need the following assumption: $E[W_{11}(.) + (\frac{n-1}{n})^2 H_{11}(.)] < C_{11}(.)$

not respond to rank feedback in $t = 1$, especially in two different directions, when they receive two different kinds of feedback. Also, conditional on differential response to feedback, the direction of response to a specific kind of feedback can tell us about the curvature of underlying status utility function $H(.)$. In the empirical framework, I will use Observation 1 to test both the existence and the curvature of $H(.)$.

1.3.2 Social Concerns

Now I introduce social concerns into the worker's utility. Conditional on rank of workers being known to each other, a worker now receives utility from his social image associated with his rank, in addition to intrinsic-status utility discussed earlier. But there are two types of social image he may care about. The first is his social image as a good worker (i.e. *social status*). Higher the rank, higher is the social status. The worker may therefore be induced to increase effort to earn higher rank. However, since a rank tournament is a zero-sum game, a higher rank for one worker means a lower rank for another. Contrary to higher ranks, lower ranks bring disappointment and shame. Thus, effort of one worker imposes a negative externality on other workers. Hence, if he tries to increase effort to earn higher rank he damages his social image as a good person or friend, the second type of social image that he cares about.[12] In fear of being taken as a self-serving person by his peers, a worker may despise getting ranked high, and either not increase effort, or in extreme cases, reduce effort to *socially conform* to that of his peers.

To introduce this trade-off between social status and social conformity, I revise the previous utility function of a worker to the following:

$$U_{it} = W(\tilde{e}_{it}) - C(e_{it}, \alpha_i) + H(z_{it}) + \underbrace{\gamma_{it} s_i H(z_{it})}_{\text{social status}} - \underbrace{\pi_{it} M(\tilde{e}_{i,t-1} - \tilde{e}^f_{i,t-1})}_{\text{social conformity}} \tag{1.4}$$

The interpretation of $W(.)$, $C(.)$ and $H(.)$ are the same as before. But now, social-image concerns introduce a social-status component to the utility by augmenting intrinsic-status function $H(.)$ with a factor $\gamma_{it} s_i$. γ_{it} represents visibility of i's rank to others - the more visible his rank is to others, stronger is the social status that he derives from his rank. I will return to a more detailed discussion on γ_{it}. $s_i \geq 0$ is the weight worker i puts on social-status.

The last component, $M(.)$, in Equation 1.4 refers to the disutility worker i gets when his effort is higher than the effort of peers who can socially punish him

[12]The worker cares about his image as a good person or friend since this can yield benefits, either monetary (e.g., borrowing money) or non-monetary (e.g., good company during work breaks).

when he shames them in ranks (let us call these peers *friends*). This disutility can come from either real punishment or, simply, his fear of punishment. Effort of peers is denoted by $\tilde{e}^f$. Disutility from shaming friends does not exist when worker i does not outperform his friends; hence $M(x) = M_1(x) = 0$ if $x \leq 0$. However, this disutility increases with the extent of out-performance; so, $M_1(x) > 0$ for $x > 0$. Since this is a cost of effort, I will let it be convex; hence $M_{11}(.) > 0$. However, because public shaming occurs only when ranks are formally made public, in period t, $\pi_{it} = 0$ if there is no public ranking, and $\pi_{it} = 1$ otherwise.

Note that in period t, a worker feels disutility from outperforming his friends in period $t-1$. This is because the information on whether a worker outperformed his friends in a given month is not available until the next month. As such, any social pressure that a worker feels from his peers is likely to be based on previous month's performance. This requires a slight change in the the timeline of events. Most of the timeline is the same as in the framework with only intrinsic-status concerns, except that now I assume that the workers expect the ranks to continue beyond $t = 1$. This deviation in timeline is necessary to let workers respond to social-conformity incentive in $t = 1$. A worker may be forgiven for outperforming his friends in $t = 0$ since there were no formal ranking in place, but he will be punished if he does it in $t = 1$ when public shaming has come into being. But if there is no ranking in $t = 2$, there will be no way to know how a worker acted in $t = 1$, and hence enforcement of social conformity is not possible.

Let us assume that the firm ranks its workers in one of two ways - either inform workers of their own ranks privately, or make the whole set of ranks public. γ_{it}, therefore, takes the following values:

$$\gamma_{it} = \begin{cases} 0 \text{ if } t = 0 \\ 0 \text{ if } t{>}0 \text{ and ranks are private} \\ 1 \text{ if } t{>}0 \text{ and ranks are public} \end{cases}$$

There are two simplifying assumptions about γ_{it}. First, visibility of ranks is the same for all workers, exogenously determined, and the workers do not affect this visibility. This is because I intend to focus on changes in workers' effort rather than their behaviour in sharing information. Second, visibility of ranks is zero unless there is a formal public ranking introduced by the firm.

For reasons already discussed earlier, π_{it} takes the following values:

$$\pi_{it} = \begin{cases} 0 \text{ if } t = 0 \\ 0 \text{ if } t{>}0 \text{ and ranks are private} \\ 1 \text{ if } t{>}0 \text{ and ranks are public} \end{cases}$$

A key assumption in this framework, however, is that social-status utility function is the same as intrinsic-status utility function $H(.)$, and it is only augmented by a scalar factor. Because both of them are status utility and both depend on how a worker perceives himself (his perceived rank), this is a reasonable assumption. Also, because we already know how their responses are driven by intrinsic-status incentives alone, the assumption makes it easier to understand how workers respond to additional social-status incentives once true ranks are revealed. As a result, I can identify the function $M(.)$ separately from social-status.

Note that, when information on ranks is kept private, the above framework with social concerns degenerates to the previous framework with only intrinsic-status incentives. On the other hand, when ranks are public, assuming an interior solution [13], we have the following observation.

OBSERVATION 2: *Let $x = \tilde{e}_{i,0} - \tilde{e}^f_{i,0}$ be the difference between a worker's own effective effort and that of his friends'. Worker i exerts more effort in $t = 1$ under public ranking than under private ranking if $x \leq 0$. More generally, there exists a value $\tilde{x} > 0$ such that, worker i exerts more effort in $t = 1$ under public ranking than under private ranking if $x < \tilde{x}$. Alternatively, he exerts less effort in $t = 1$ under public ranking than under private ranking if $x > \tilde{x}$.*

The proof is provided in the appendix. The intuition is the following. When information on rank is made public, the visibility of ranks increases. This, in turn, introduces social-status utility attached to ranks. Making ranks public, however, also switches on public shaming. This introduces disutility from ranking higher than friends. But when a worker is not ranked higher than his friends, he responds only to social-status incentives, and increases effort relative to the case under private ranking. More generally, as long as a worker's rank distance with his friends is not too high, his marginal social-status utility is higher than his marginal disutility from outperforming friends; so, he increases effort. On the contrary, when his rank distance with his friends is too high, the marginal disutility from outperforming

[13] For an interior solution, I need the following assumption: for any given i, $\frac{\partial^2 M(.)}{\partial e^2_{it}} > s_i \frac{\partial^2 H(.)}{\partial e^2_{it}}$.

friends overtakes marginal utility from social status; so, he decreases effort.

1.4 The Experiment

1.4.1 The Design

Along the line discussed in the theoretical framework, I designed and implemented a randomized experiment at the sweater factory described in Section 1.2. The intervention provided treated workers with relative ranks based on their previous month's performance. The research team in the field along with supervisors of the blocks delivered the ranking information to each worker through individually addressed letters at the end of every month, for 9 months after the intervention commenced. The control group also received letters, but no information on ranks. The content of these letters is discussed in Section 1.4.4.

There were two treatment groups. The first, *Private Treatment* Group, received letters that informed workers of only their own ranks, and no one else's; the ranks were computed among workers in this treatment group only. Because the ranks were private, no extrinsic incentives were involved.[14] Hence, the Private Treatment allows me to understand how revelation of true ranking information affects workers because of their intrinsic status incentives alone. Conversely, the second treatment group, the *Public Treatment* Group, received ranking information in such a way that the ranks of all workers were made known to each other; again, these ranks were computed among workers in this treatment group only. The second treatment induced response from intrinsic-status concerns just as the first did, but because the ranks were now public, this also induced response to social concerns. Comparing the Private and Public Treatment groups allows me to isolate the effect of social concerns.

In terms of the theoretical framework, the experiment does the following. First, by revealing information on true ranks to Private and Public Treatment workers, the experiment eliminates δ_{i0} from their perceived ranks. Second, in the Public Treatment, it increases the visibility of rank information, and hence switches on social concerns among workers only in the Public Treatment. To be more concrete, $z_{it}(.)$ is now redefined as the following:

$$z_{it}(.) = \left[\tilde{e}_{i,t} - \frac{1}{n}\sum_j \tilde{e}_{j,t}\right] + \delta_{i0}(1 - v_{it})$$

[14] A valid concern here is that workers in the Private Treatment group may have shared rank information among themselves, essentially opening up the door to extrinsic incentives. I will discuss this issue in Section 1.7.

where v_{it} , treatment status of worker i, takes the following values:

$$v_{it} = \begin{cases} 0 \text{ if } t = 0 \text{ and } i \in \{Control, Private, Public\} \\ 0 \text{ if } t > 0 \text{ and } i \in \{Control\} \\ 1 \text{ if } t > 0 \text{ and } i \in \{Private, Public\} \end{cases}$$

In addition:

$$\gamma_{it} = \begin{cases} 0 \text{ if } t = 0 \text{ and } i \in \{Control, Private, Public\} \\ 0 \text{ if } t > 0 \text{ and } i \in \{Control, Private\} \\ 1 \text{ if } t > 0 \text{ and } i \in \{Public\} \end{cases}$$

Thus, the goal is to identify the presence and impact of intrinsic-status incentives $H(z_{it})$ by experimentally changing the value of perceived ranks $z_{it}(.)$ among treated workers. To switch off social concerns, γ_{it} and π_{it} are set to zero by making information on ranks private in Private Treatment. Since control group workers do not receive information on their true ranks, $z_{it}(.)$ do not change for them. Therefore, any differential response in Private Treatment, relative to Control group, will be driven by changes in intrinsic status incentives induced by changes in perceived rank $z_{it}(.)$.

On the other hand, the value of γ_{it} and π_{it} are experimentally changed to one for Public Treatment workers by making their rank information public. In addition to intrinsic effect from changes in perceived rank, this now switches on both social-status and social-conformity mechanisms. Any differential effect in Public Treatment, relative to Private Treatment, will be driven by the two social concerns induced by increased visibility of ranks.

Observation 1 from the theoretical framework suggests that a worker will respond differently to the intervention depending on whether he overestimated or underestimated his rank prior to the intervention. In other words, how a worker responds depends on δ_{i0}, the ex-ante noise in his perceived rank. To measure δ_{i0}, I used a baseline survey before the intervention to collect information on what each worker expected his rank to be. The difference between his expected rank and his true rank provides a measure of δ_{i0}. This allows me to understand whether the true rank information revealed through the experiment conveyed a positive or negative surprise to a given worker, and also to empirically test Observation 1.

Next, to disentangle social-status and social-conformity effects, it is necessary to first identify a reference group that workers may feel social pressure to conform to.

In the theoretical framework, this reference group is denoted by f in the superscript of $\tilde{e}^f$. To do this, I compiled a detailed map of the existing social network at baseline. I mapped the network in multiple dimensions. To be concrete, I collected information on who a worker socialized with outside the factory, who he talked to within the factory, and the administrative block to which he belonged. Potentially, any of these, along with the whole workforce on the floor, could define a worker's reference group to which he might conform. This, along with the fact that the intervention was continued for 10 months, allow me to check how within-worker behavior varied across months in response to ranks of his peers in his reference group, and, in the process, to test Observation 2.

One potential concern in this design is spill-over effect. We may be particularly concerned with spill-over from Public Treatment to Private Treatment, since the former might induce a norm of sharing information in the latter, making the latter less private. To check if there was any spill-over effect, the treatments were stratified across blocks. Anecdotal evidence from the factory indicated that the workers were more closely connected socially to workers within their own blocks, and hence a block encompassed most of a worker's peer connections, regardless of how those connections are defined (e.g., social proximity vs. spatial proximity). Before randomly assigning workers into experimental arms, first we randomly selected all 15 blocks of the floor into one of two categories, which I refer to as Category A and Category B. In Category A, 43.33% of operators were assigned to Private Treatment and 23.33% to Public Treatment. In Category B, the public/private weights were reversed. The control group consisted of one-third of the block operators in all blocks; overall, one-third of operators were in each of the two experimental groups. For each treatment group, the stratification created an exogenous block-level variation in the exposure each treatment group had to the other. The exposure at the block level captured both social proximity and spatial proximity, and helps to identify potential spill-over effects from one treatment group to the other.

Following random assignment of the blocks into the two categories, we[15] held a *public lottery* within each block. We presented each operator with a bag of tokens with hidden numbers written on them: '1', '2', or '3'. The total number of operators in the block and whether the block was in Category A or B determined the composition of tokens for a given block. We explained to the operators that they would receive monthly information based on their production, and that the type of information they would receive depends on the number they picked. A public lottery eliminated the possibility of behavioral responses stemming from suspicions on how

[15] I switch to 'we' to include the field team members of this study.

they became inducted into one group and not another, but it precluded stratifying treatment on any characteristics other than block.

Table 1.1: Key Descriptive Statistics

	(1) Control	(2) Private	(3) Public	(4)	(5)
Block Category	n	n	n	Total	
Category A (Private Intensive)	59	71	39	169	
Category B (Public Intensive)	66	46	85	197	
Total	125	117	124	366	
Production	Mean	Mean	Mean	(1)-(2)	(1)-(3)
Pre-Intervention Monthly Production Wage (Tk.)	10504.59	10453.06	10504.44	51.53	0.16
Pre-Intervention Mean Daily Wage (Tk.)	386.34	384.79	386.11	1.55	0.22
Pre-Intervention Monthly Attendance (days)	27.05	27.00	27.08	0.05	-0.02
Age on Jan 1, 2015 (years)	29.61	29.44	29.90	0.17	-0.29
Length of Tenure on Jan 1, 2015 (years)	4.32	4.47	4.28	-0.15	0.05
Social Network	Mean	Mean	Mean	(1)-(2)	(1)-(3)
# of Operators in Block (Drafted)	24.55	24.61	24.63	-0.05	-0.08
# Peers (from block) Socially Interacts with	8.66	7.99	8.98	0.67	-0.32
# Peers (from block) Talks with >=3days/wk	12.65	13.34	14.33	-0.69	-1.68*
	n	n	n	Total	
Chose Competitive Version of Ball-Bucket Game	56	58	51	165	
Chose Non-Competitive Version of Ball-Bucket Game	69	56	73	198	

Note: The table reports key descriptive statistics for each experimental group. The last two columns report the differences in these statistics between control and the treatment groups. The difference is then tested against the null that it is zero. *, **, *** indicate that the null is rejected at 10%, 5% and 1% significance level respectively.

The top panel of Table 1.1 shows the final distribution of operators across experimental arms. The control group consisted of 125 workers, the Private Treatment group consisted of 117 workers, and the Public Treatment group consisted of 124 workers.[16]

We made it clear to all the operators on the floor that their rank performance would not be rewarded or punished in any other way. There were no monetary rewards for better performance (beyond the higher wages implied by piece rates). There was also no system for promotion of any type. Nonetheless, workers might still have been under the impression that the best performances would somehow be rewarded by the management, and that bad performances would be punished (e.g., they would be fired). In this case, any response from either of the treatment

[16]A slight deviation from one-third of operators in each arm resulted from rounding up of the number of operators for each group in each block.

groups could also be driven by incentives for rewards or by fear of punishment. Although such beliefs could not be eliminated completely, even if they existed, they should have existed only in the first months of the treatment, after which workers would have realized that no such external punishments or rewards were forthcoming. Continuing the treatment for 10 months allows me to check whether such concerns for punishment or reward matter.

1.4.2 Timeline

In October 2015, we conducted a baseline survey; then, in January 2016, we drafted 366 available workers into experimental groups. It was also only during the drafting that we first informed the workers about the intervention that would follow. The top management of the factory agreed to implement the experiment as its own management practice, and so they passed down the orders to the production manager on the floor, who in turn conveyed the message to the supervisors. Thus, the experiment was introduced as a new management practice to the floor, rather than as an experiment by an external group of researchers. We delivered the first set of treatment letters in early February 2016, and the final set in early October 2016, giving us a total of 9 treatment months (excluding January 2016 when the workers did not receive any rank information but their performance counted towards rank computation). At no point did we mention an end date to the experiment.

Over July-August 2016, the factory hired 95 additional operators to the existing workforce. We also drafted these new operators into the experiment later. However, these workers started working at the factory knowing that there was a ranking system already in place. Hence, their responses to rank incentives might differ from those of the already existing workers. Although this might be interesting to consider in its own right, I leave them out from the analysis.

1.4.3 Rank Calculation

We computed the ranks provided to treatment workers in five steps. First, for each style and size produced by a worker in the previous month, we computed an average production time per set of sweater panels. In the second step, we compared this average time with the time put in by all the other workers in the same treatment group who also worked on the same style and size, to compute a *style-rank* for each style and size; a higher numerical rank would imply a worse performance. In the third step, for each worker, we normalized each of all the worker's style-ranks by the highest rank value for each of those styles (the worst rank in the treatment group).

In the fourth step, we weighted the normalized-style ranks by the share of a given style in the worker's total production in the previous month. Then, we summed all the normalized-and-weighted-style ranks for each worker, to produce a weighted average of normalized-style ranks. Finally, in the fifth step, we produced a final rank for each worker by comparing this weighted average of style ranks with that of others in the same treatment group.[17] In the rare instances when two or more workers had the same value for weighted average style-ranks, we gave them the same final rank.

It was important that workers would not be able to compute and compare ranks among themselves when they were not meant to (i.e. in the control group, or across Private and Public Treatments). Because the information on the actual production times was recorded centrally at the Distribution Section, the workers had no access to this information, and hence would be unable to compute their ranks independently. Nonetheless, there could be concerns that because the workers were paid on piece rates, information on total wages could help them deduce their ranks when ranking information was not available to them. However, ranks based on total production wages would not predict time-based ranks for two reasons. First, total production wages depended on the piece rates, which in turn, varied across styles. Moreover, the workers were aware that the piece rates did not always reflect the complexity or production time of a given style. Second, in a typical month, a worker worked on four different styles, and these styles would not necessarily match the set of styles produced by another worker. Both of these reasons combined made it more difficult for workers to use wages to deduce time-based ranks.

In addition to the main components (front, back, and sleeve panels), a typical sweater requires a few supplementary parts, for example necks. These supplementary parts are assigned separately from the main panels, and usually have piece rates that allow workers to earn more than they would with other parts. Moreover, because these parts are small, one worker can produce a big batch of them in a short time. So, for a given style of sweater, it is not practical to assign these to more than a few workers. Consequently, the production of these parts for different sweater styles is rotated across different workers in different months, for fairness purposes.

We computed the actual ranks from only main sweater panels, and excluded any supplementary parts that a worker might have worked on. This was necessary because all the supplementary parts of a given sweater style would be produced by a small number of workers, all of whom may not be in the same experimental group, making it impossible to compute style ranks for these parts. Leaving these out

[17]This computation is shown in detail in the Appendix.

meant that the total production wages (which covered all styles and parts) were to yet another degree out of step with any independent predictions that workers might have attempted to make regarding time-based ranks.[18] Moreover, the production time used in ranking computations excluded pre-authorized leaves, but included all unauthorized absences. While this approach served to punish workers for taking unauthorized absences, including these absent times also distanced wage-based ranks from time-based ranks. The treatment letters contained all the details about how ranks were calculated, and this information was repeated every month to serve as reminders.[19]

Note that we did not provide workers with information on the actual time they took to produce sweaters. Hence, a worker from one treatment would not be able to compare himself with a worker from the other treatment. However, not knowing the distance needed to cover to achieve a better rank could have potentially discouraged them from trying in the first place. So, I followed Barankay [2012] and provided workers with information on what ranks they could achieve if they improved their average production time by 5 percent, 10 percent, and 20 percent. This information gave them an idea of how harder they would need to work, but they would not be able to use it to compare themselves with other workers from a different treatment group.

To sum up, the ranks computed from time were different from the ranks that could be computed from wages, and thus prevented workers from predicting their ranks when they were not meant to. Figure 1.1 shows how these two ranks correlate with each other. Because both are measures of productivity, they should be positively correlated, as indeed they are. Nonetheless, there is also sufficient noise for wage rank not to be able to precisely predict time-based rank.

[18]Consequently, workers who worked on only supplementary parts in a given month were excluded from rankings.

[19]In addition, following the delivery of letters in the first few months of treatment, a member of the research team was available in each block at designated times set in advance to answer any question that the workers might have. The letters also announced this time in advance.

Figure 1.1: Comparison of Wage based Ranks to Time Based Ranks

Note: The figure shows how ranks computed from mean production wage per day in each month (horizontal axis) correlates with actual time based ranks that were used in treatment letters (vertical axis). '20160X' refers to calendar month 'X' of year 2016. Time based ranks were fairly correlated with wage based ranks, as the wages were based on piece rates, and hence reflects actual productivity to some extent. Nonetheless, wage based ranks would not be able to precisely predict time based ranks because of additional noise introduced by how time based ranks were computed.

1.4.4 The Treatment Letters

As stated earlier, we delivered information on ranks to the treated workers through monthly letters. Prior to the intervention, the factory recorded only the dates a job was distributed and received, not the precise time of the day. The factory started recording the time to help compute ranks for this experiment. To negate any potential responses from the treatment groups just because of receiving the letters, or from the perception that they were being observed, all the workers in the control group also received letters at the end of every production month. These letters contained the following trivial information:

i Number of sweaters the worker produced in the previous month (broken down into styles and sizes)

ii Total time taken to produce the sweaters

iii Names of all the workers in his group, along with their card numbers and block numbers, sorted by first block and then card numbers

While (i) above was already known to the operators, (ii) was new information and important because it helped them to feel included in the experiment and negated any behavioural responses from treatment groups stemming from the feeling that their productions were being timed.

On the other hand, each worker in the Private Treatment group received a letter with the same information as those in the control group, plus the following:

iv The worker's relative rank (in the previous month) among all the workers in the group, and the total number of workers

v The lowest rank in the group

vi What the worker's rank would have been had he improved his time by 5 percent, 10 percent, and 20 percent, ceteris paribus

While (iv) was the core treatment information that helped workers to identify their positions in the distribution, (v) was important because more than one worker could jointly hold ranks; thus, the total number of workers in that case would not be sufficient to identify the worker's relative position in the distribution. Moreover, this could vary across months because workers left or joined the factory, or simply, some workers might not have been ranked in a given month for reasons discussed earlier. Finally, (vi) helped workers to understand how much they needed to improve to progress in the ranking.

Each worker in the Public Treatment Group received the same information as those in the Private Treatment group, except that for the former, the names of all the other workers in his group appeared with their respective ranks; this rank was also the variable by which we sorted the list.

To sum up, the control group received letters with information that was not highly informative, but served to produce the same potential effect of receiving letters or being timed as they would in the treatment groups. Treatment groups received the same information as the control group, but the former also received information about their ranks, either privately or publicly, depending on the treatment. The difference between the control group and the treatment groups lies in the additional ranking information received, while the difference between the two treatment groups (Private and Public) lies in whether or not other people also knew about their ranks.

1.5 Data

The data I use for this paper come from two key sources: administrative data from the factory, and a baseline survey conducted in October 2015 before we drafted workers into the experiment. Below is a brief discussion of the key data from these sources.

1.5.1 Administrative Data

The administrative data from the factory provide detailed information on individual worker-level production wages, attendance, and breakdown of production into sweater styles and corresponding quantities, all compiled at the month level. These are available from January 2013 to October 2016. Starting from January 2016, we also collected the time it took for each operator to complete each of his jobs; we used this to compute the ranks.

The second panel of Table 1.1 shows the mean values for monthly wage, total days of attendance, average daily wage (total wage per attendance day, which I use as the outcome variable for analysis), age, and tenure at the start of the experiment. Columns 1-3 show the means for each experimental arm, while columns 4 and 5 show the difference in means between the control group and each of the treatment groups. These differences are statistically insignificant at traditional significance levels for all of the variables, implying that the groups were well balanced on these characteristics.

Administrative data also contained information on the block in which each worker belonged for each month of the sample period. The data show that it was extremely rare for a worker to change blocks. This implies that workers within a given block had worked in close proximity to each other at least since the start of the sample period in January 2013, or since they joined the factory, whichever came later.

1.5.2 Social Network

We mapped the social network of workers in multiple dimensions. One definition of the network is simply the block to which a worker belongs. However, a more relevant measure in the context of social conformity is the network that can impose social punishments on a worker. Hence, the baseline survey collected information on the peers with whom a worker socialized outside the factory, and those with whom the worker regularly interacted while inside the factory. Because anecdotal evidence from the factory indicated that the workers were more closely connected to workers within their own blocks, and because it was impractical to ask about each of the 365 other workers individually, we focused relatively more on the networks within a worker's block. Specifically, we asked the workers to consider each of the other workers in their block, name by name, and tell us how frequently they talked to them and whether they socialized with them outside the factory.

It was not possible to perform the same exercise for all the other workers on the floor, because of the large length of time it would have taken to complete the survey. Instead, we asked them to name 10 workers from outside their own block to whom they talked frequently, or with whom they socialized. Of the 363 workers completing the network survey, 91 said they did not socially engage with anyone outside their block, while only 16 named 10 peers. The left panel in Figure 1.2 shows that, from within a block, the workers socialized with approximately eight co-workers on average; outside the block this average was approximately three.[20] The right panel of Figure 1.2 shows that the differences in number of friends is stark when computed as a share of total workers in the block or floor.[21] Consistent with anecdotal evidence, this suggests that the workers were more socially connected with peers inside their block than with those outside their block. Hence, for the rest of the paper, I will focus on this within-block network.

The Social Network panel of Table 1.1 shows that the number of within-block peers with whom a worker socialized outside the factory is well balanced across the experimental groups. Table 1.1 indicates that the number of block peers with whom a worker conversed more than three days a week was higher in the Public Treatment group than in the control group. While there is slight imbalance in this measure, all the other observed characteristics are well balanced on average. An F-test of the social-network measures as a whole shows that they are jointly insignificant in predicting treatment status either as a combined treatment group or as separate groups (Private and Public).

1.6 Main Results

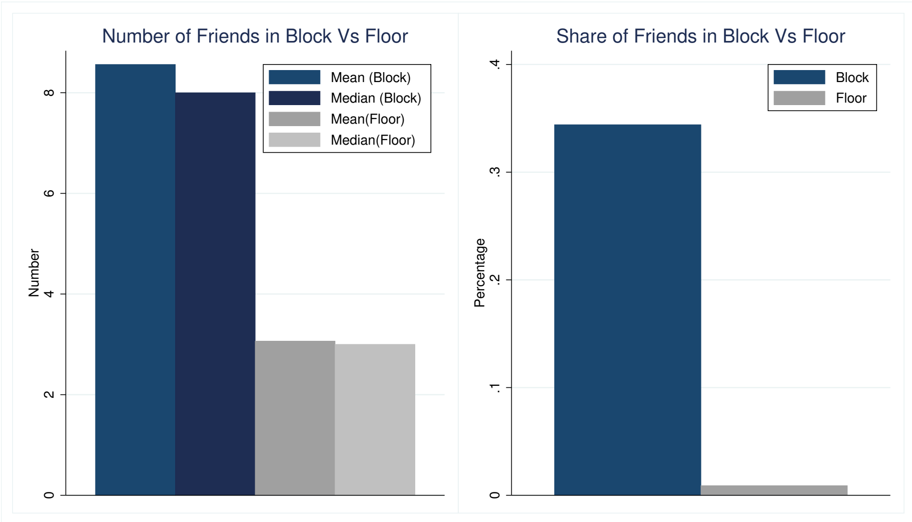
In line with the theoretical framework discussed earlier, this section reports the main empirical results of the treatment effect. I will start with the average baseline effect, then test the effect of intrinsic status concerns (Observation 1), and finally move on to understand the effect of social concerns (Observation 2).[22]

[20] Conditional on naming fewer than 10 peers, this number is 2.74.

[21] The average number of workers in a block was 25, and hence that for the rest of the floor was 341.

[22] The pre-analysis plan mentioned all tests of the various heterogeneity done here.

Figure 1.2: Social Network Within Block and Outside Block



Note: Left panel of the figure above plots the mean and median number of friends workers have within their block, and rest of the floor outside their block. Instead of absolute numbers, the right panel shows the share of workers that a worker is friends with within his block, and rest of the floor. The two panels show that workers are more likely to have social ties with workers within block rather than outside block.

1.6.1 Average Treatment Effect

I take advantage of the availability of long-period pre-intervention data for each worker (because workers had been at the factory for a long time) and use a difference-in-difference (DID) strategy to identify the treatment impact.[23] The key baseline specification for our analysis is therefore:

$$Y_{it} = c + \alpha_i + \tau_t + \beta(Treatment_i * Post_t) + \lambda' X_{it} + \eta_{it} \tag{1.5}$$

where Y_{it} is the outcome variable of interest, and almost always the logarithmic transformation of mean daily wage earned by worker i in month t, and which I use as the measure of worker productivity; mean daily wage is computed as total wages earned from producing all sweater parts divided by the total number of attendance days; α_i is worker fixed effect; τ_t is year-month fixed effect; the DID estimate of treatment effect, β, is the key coefficient of interest; and X_{it} is a vector of additional individual specific time-variant controls used in certain specifications. Given the limited number of workers in each experimental group, the DID approach strengthens the identification of treatment effect. Logarithmic transformation of

[23] An alternative would be Ancova analysis (McKenzie [2012]), but variance calculations show that DID and Ancova analysis are equally efficient with the data in this context.

the outcome variable helps to interpret the coefficients as percentage change in the outcome variable.

It is worth elaborating on X_{it} a little more. As mentioned earlier, production wages not only depend on how productive a worker is, they also depend on the piece rates of the styles on which a worker works. The piece rates may not always reflect the complexity of the styles, and hence wages alone are a noisy measure of productivity. Conversely, the average time a worker takes to produce a sweater would be a more precise measure of productivity (which is what was used to compute ranks), but these times are not available for the pre-intervention period. Hence, I use total production wages from all sweater parts and total attendance days to obtain a measure of their mean production wage per working day. This serves as the next-best measure of productivity for both before and after the experiment. However, to reduce the noise stemming from workers producing various styles with various piece rates, in X_{it} I include style fixed-effects. These style fixed-effects are different for each sweater style and part; thus, they not only control for the varying complexities of sweaters, but also for whether a worker worked on supplementary parts, which usually have higher piece rates. I also include here, depending on specifications, the monthly block size, which varies across months as workers quit or join the factory. However, for a given month it will be the same for all workers from the same block. So, strictly speaking, this varies only at the block and month levels. Ideally, I might have controlled for this with a block fixed-effect, but there is little movement of workers across blocks.

I restrict the sample of workers to those who had been at the factory from before the start of the experiment. In other words, I exclude the 95 workers who were hired in the middle of the intervention because they may respond differently to the intervention than the others.

To keep with the standard approach in this literature, and to check how the treatment effects in this paper compare to those found in the existing literature, I start by estimating the average treatment effect of the intervention. Table 1.2 shows the baseline DID estimates of the average treatment effect. In columns 1 and 2, the treatment groups are pooled together. Column 1 is the simplest baseline specification without fixed-effect and other controls; column 2 introduces worker, year-month, and style fixed-effects, as well as block-size controls. Columns 3 and 4 correspond to columns 1 and 2, respectively, but the former split the treatment groups into Private and Public Treatment groups. Regardless of specifications, columns 1-4 show that, on average, the treatments had no effect. Not only are the estimates statistically insignificant, they are also small in magnitude. A small difference emerges between Private and Public Treatment effects, indicating that the workers in the Public Treatment might have performed worse than those in the Private; however, the difference is statistically insignificant and, hence, inconclusive at this stage.

Table 1.2: Baseline Results

	(1) Ln(Wage)	(2) Ln(Wage)	(3) Ln(Wage)	(4) Ln(Wage)
[A] Treatment * Post	0.000961	-0.000955		
	(0.00329)	(0.00807)		
[B] Treatment	-0.00497			
	(0.0171)			
[C] Private * Post			-0.000519	0.00114
			(0.00391)	(0.0103)
[D] Public * Post			0.00238	-0.00292
			(0.00621)	(0.00858)
[E] Post	0.0278		0.0278	
	(0.0355)		(0.0355)	
[F] Private			-0.00753	
			(0.0200)	
[G] Public			-0.00255	
			(0.0200)	
[H] Block Size		6.65e-05		7.31e-05
		(0.00121)		(0.00121)
Constant	5.918***	5.344***	5.918***	5.344***
	(0.0291)	(0.0450)	(0.0291)	(0.0455)
Observations	14,263	14,251	14,263	14,251
Adj. R-Sq.	0.002	0.796	0.002	0.796
N(Worker)	366	366	366	366
N(Months)	46	46	46	46
FE: Worker, Year-Month, Style	NO	YES	NO	YES

Note: Dependent variable is log of mean daily wage. In Cols. 1-2 Private and Public treatment groups are pooled together as one Treatment group, while in Cols. 3-4 they are tested separately. Pre-treatment months are January 2013 - January 2015, while post-treatment months are February 2015 - October 2015. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. The table shows that on average, treatment effect in either treatment is close to zero.

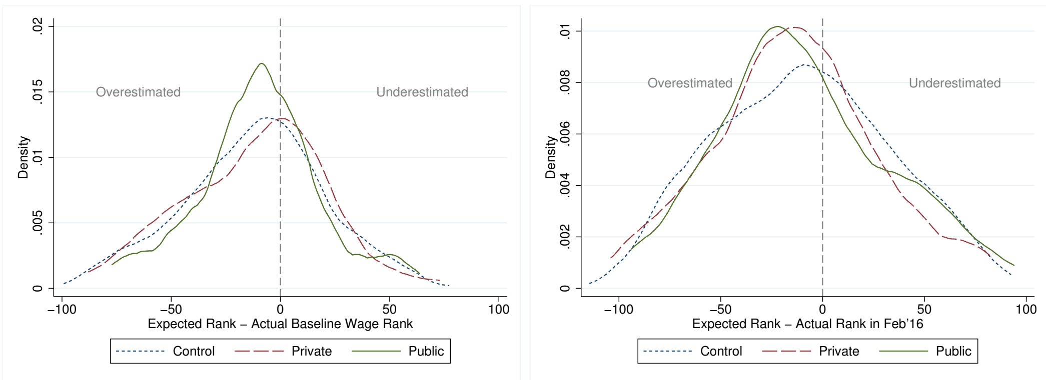
The finding of an overall zero average treatment effect, particularly from Private Treatment, is similar to that found in studies such as Blader et al. [2014]. But, does this arise because the workers did not care about the ranks at all, or were there heterogeneous responses that offset each other? Indeed, the fact that the overall treatment effect is close to zero is not entirely surprising. The theoretical framework of this paper did suggest that the treatment effect would vary depending on whether a given worker had overestimated or underestimated his rank prior to the intervention, and also on the shape of the underlying status-utility curve. Such heterogeneous responses could offset each other and lead to an overall zero effect. So, in the following section, I empirically test theoretical Observation 1.

1.6.2 Treatment Effect from Intrinsic Status Concerns

As noted in Section 1.4, during the baseline survey prior to intervention, we asked workers what they thought their ranks were among all the workers in the whole knitting section. After we randomly grouped them into their corresponding experimental groups, the reported ranks, normalized with respect to the sizes of their experimental groups, serve as an estimation of the ranks they would expect to receive from the treatment (their *Expected Ranks*). Ex post, we revealed their true ranks to them in the first month of treatment. The difference between expected ranks and true ranks as seen on the treatment letter is the surprise the workers received. Hence, this difference serves as a measure of δ_{i0} in the theoretical framework. A positive difference $\delta_{i0} > 0$ for a given worker would imply that the worker had *overestimated* his rank earlier, and then received *negative* feedback from the treatment; a negative difference $\delta_{i0} < 0$ would imply that the worker had *underestimated* his rank earlier, and then received *positive* feedback.

It is worth noting one fine point. When we asked a worker about his expected rank during the baseline survey, we asked him to rank himself based on the wages of the previous three months. On the other hand, the ranks in treatment letters were computed from actual production time in immediate previous month. Hence, reported expected rank and true rank in the treatment letter differ in two dimensions. First, the expected rank was based on wages, while true rank was based on time. Second, expected rank relates to a worker's expected rank based on the three months preceding the baseline survey; but true ranks in a given month relate to work in the immediate previous treatment month. Hence, computing δ_{i0} in the way described in the previous paragraph assumes that the expected rank from the baseline survey nonetheless reflects the rank that the worker would expect to receive in the treatment letters. Because rank based on long-term average wages (three months in this context) is a fairly precise measure of productivity, this is a reasonable assumption. Indeed, distribution of δ_{i0} computed as a difference between expected rank and *true wage rank at baseline* looks very similar to that computed as a difference between expected rank and *time-based rank from the first treatment letter*; it is shown in Figure 1.3.

Figure 1.3: Difference in Expected Rank and True Rank



Note: The first panel plots the distribution of difference between expected ranks as reported by workers during baseline survey and their actual wage-based ranks during the baseline survey. The second panel plots the distribution of difference between expected ranks as reported by workers during baseline survey and the actual rank provided through treatment letter in first treatment month. A negative value for this difference implies a worker overestimated his rank, while a positive value implies he underestimated it. The distribution shows that workers were likely to both overestimate and underestimate their ranks, with a heavier mass for the first group. Separate distribution plots for control and treatment groups show that they are largely balanced across experimental arms.

Table 1.3 shows the empirical test of Observation 1. I focus only on the Control and Private Treatment groups to understand intrinsic-status concerns because, in the Public Treatment group, social concerns muddle the mechanism.[24] The workers are now split into two subsets. Column 1 refers to all the workers who received positive feedback through the treatment letter in the first month. Column 2 refers to those who instead received negative feedback.[25] Control-group workers are similarly split into these categories. The control-group workers never received any ranking information in practice, but I can nonetheless compute their ranks and, hence, the

[24]The results for Private Treatment are almost identical when Public Treatment workers are also included in the sample.

[25]The fact that there were more workers who had previously overestimated their ranks (n=139) than who had previously underestimated their ranks (n=82) reflects the distribution plots in Figure 1.3. Also, this is consistent with findings in existing literature that suggest that people usually overestimate their own performance (e.g. Svenson [1981]; Meyer [1975]).

feedback they would have received had they also been treated. Control subsets serve as more appropriate counterfactuals compared to the whole control group because they control for any unobserved characteristics that determine whether a worker underestimates or overestimates his rank. Also, note that there is no statistical difference between pre-intervention wages of private- and control-group workers, even after splitting the workers into the two subsets.

The first two columns in the first row of Table 1.3 show that there was indeed a differential response to the type of feedback received by workers. Workers who were told that their effort yielded much more status return than they had previously expected increased effort by about 2.5 p.p., while those who were told that the return was lower than they had previously expected decreased effort by a little more than 1 p.p. However, the difference between the two coefficients is more important. The difference in these coefficients reported in column 3 is almost 4 p.p. in size and statistically significant. I do the same analysis in columns 4-6, but include additional controls. The results are similar.

The findings in Table 1.3 provide support to this paper's theoretical framework in two ways. First, the fact that we see a differential response to feedback suggests that workers do respond to changes in their perceived rank, z_{it}, that were induced by the experiment. Implicitly, this validates the presence of the function $H(.)$ in worker's utility function; that is, workers do care about intrinsic status. Second, the fact that workers responded positively to positive feedback and negatively to negative feedback suggests that the underlying status utility from ranks is convex in nature, that is $H_{11}(.) > 0$.[26] Because different workers respond in two opposite directions, the net average treatment effect is close to zero, as was seen in Table 1.2.

I note here that workers were split into two categories based on the feedback they received in the first treatment letter; but the treatment effect was estimated from all the subsequent treatment months considered. Because workers received ranks in every month of the treatment period, it is possible that they also responded to rank information in subsequent months after the first treatment month. I return to this issue in Section 1.6.4.

[26]Inadequate sample size prevents me from testing whether there is a point of inflection in the underlying status-utility curve.

Table 1.3: Motivation/Demotivation Effect from Revelation of True Ranks

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Wage)	Ln(Wage)		Ln(Wage)	Ln(Wage)	
	Positive Feedback ($\delta_0 < 0$)	Negative Feedback ($\delta_0 > 0$)	Cols [1] - [2]	Positive Feedback ($\delta_0 < 0$)	Negative Feedback ($\delta_0 > 0$)	Cols [3] - [4]
Private * Post	0.0269***	-0.0130**	0.0399***	0.0230*	-0.0139	0.0398**
	(0.0077)	(0.0057)	(0.0127)	(0.0119)	(0.0103)	(0.0164)
Post	0.0168	0.0350				
	(0.0353)	(0.0356)				
Private	-0.0129	-0.0041				
	(0.0338)	(0.0276)				
Observations	3,279	5,298	8,577	3,278	5,293	8,571
Adj. R-Sq.	0.003	0.003	0.007	0.785	0.779	0.782
N(Worker)	82	139	242	82	139	221
N(Months)	46	46	46	46	46	46
Constant	YES	YES	YES	YES	YES	YES
FE: Worker, Year-Month, Style	NO	NO	NO	YES	YES	YES
Additional Control: Block Size	NO	NO	NO	YES	YES	YES

Note: Dependent variable is log of mean daily wage. *Positive Feedback* (*Negative Feedback*) refers to a worker whose rank in the first treatment month was higher (lower) than his expected rank. δ_0 refers to noise parameter used in the theoretical framework; it refers to the noise in a worker's perceived rank prior to the experiment. Workers from Public Treatment are excluded from the sample. Sample period contains 46 months (pre-treatment: January 2013 - January 2015; post-treatment: February 2015 - October 2015). Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. The table shows that workers who received positive feedback in the first treatment month increased effort in subsequent months, while those who received negative feedback decreased effort.

1.6.3 Treatment Effect from Social Concerns

Let us now try to understand the treatment effect in Public Treatment. Public Treatment differs from Private in that it introduces social concerns in addition to intrinsic-status concerns. Social concerns, in turn, consist of both social-status and social-conformity incentives. To disentangle social concerns into social-status and conformity incentives, I first check whether there were indeed preferences for such conformity in the Public Treatment group. If a worker faced social pressure to conform to his peers, we would expect the worker to reduce effort when he found himself ranked relatively better than his peers who were *also in the Public Treatment group*. But who were the peers with whom he would care to conform?

Giving in to conformity pressure and reducing effort is costly to the workers in this experiment. Because they were paid piece rates, reduced effort also implied reduced income. So, for a worker to conform in effort, the return he would receive from conforming would have to offset the income he would lose. Therefore, a strong candidate for the reference group are the co-workers with whom a worker socialized (i.e. *friends* of the worker), because they had the power to impose social costs on him should his ranking shame them. Hence, if there were any effect from conformity, it would be strongest with respect to friends. As mentioned earlier, here I consider only friends from one's own block, because baseline data show that workers were more likely to be friends with peers from the same block.

If the social pressure to conform was present, then it would be felt by workers who were relatively more productive than their friends; on the other hand, workers who were relatively less productive than their friends would feel no such pressure. This is precisely what I test in Table 1.4. Column 1 is simply the average treatment effect in the two treatment groups. In column 2, I test how publicly ranked workers who were relatively more productive than their friends from the same block and the same treatment responded after the introduction of the treatment. To do this, I split the publicly ranked workers into two groups - workers whose baseline productivity was higher than the median among their friends, and workers whose baseline productivity was lower than the median among their friends. Baseline productivity is measured by the average daily wage from the whole pre-treatment period. Median is computed from a group that consists of a worker himself and all his friends from the same block and the same treatment.

Table 1.4: Conformity towards Friends - With Baseline Productivity

	(1) Ln(Wage)	(2) Ln(Wage)	(3) Ln(Wage)	(4) Ln(Wage)
[A] Private * Post	-0.000519	-0.0030	-0.0004	0.0040
	(0.00391)	(0.0046)	(0.0064)	(0.0106)
[B] Public * Post	0.00238	0.0274**	0.0285***	0.0128
	(0.00621)	(0.0109)	(0.0106)	(0.0085)
[C] Private * Post * 1(Base. Prod. > Median among Friends)			-0.0059	-0.0118
			(0.0144)	(0.0185)
[D] Public * Post * 1(Base. Prod. > Median among Friends)		-0.0594***	-0.0626***	-0.0453***
		(0.0155)	(0.0133)	(0.0146)
[E] Post	0.0278	0.0224	0.0213	
	(0.0355)	(0.0364)	(0.0361)	
[F] Post * 1(Base. Prod. > Median among Friends)		0.0122	0.0153**	0.0194*
		(0.0075)	(0.0071)	(0.0111)
Observations	14,263	14,263	14,263	14,251
Adj. R-Sq.	0.002	0.096	0.097	0.782
N(Worker)	366	366	366	366
Constant	YES	YES	YES	YES
FE: Worker, Year-Month, Style	NO	NO	NO	YES
Other Controls	NO	NO	NO	YES
B + D		-0.0321***	-0.0340***	-0.0325***
		(0.0089)	(0.0071)	(0.0104)
Social Conformity Effect: (B + D) - (A + C)			-0.0278**	-0.0246**
			(0.0118)	(0.0125)

Note: Dependent variable is log of mean daily wage. *1(Base. Prod. > Median among Friends)* is a dummy variable that takes the value 1 if pre-treatment productivity of a worker (measured as mean daily wage over the whole pre-treatment period) is higher than the median pre-treatment productivity among all of his friends (the worker himself included) who are from the same block and in the same treatment. Pre-treatment months are January 2013 - January 2015, while post-treatment months are February 2015 - October 2015. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively.

Table 1.5: Conformity towards Friends - With Information on Ranks

	(1) Ln(Wage)	(2) Ln(Wage) Pos. Feed.	(3) Ln(Wage) Neg. Feed.	(4) Cols [2] - [3]	(5) Ln(Wage)
[A] Private * Post	-0.0028	0.0297*	-0.0211**	0.0519**	-0.0071
	(0.0083)	(0.0161)	(0.0103)	(0.0188)	(0.0090)
[B] Public * Post	0.0088	0.0306*	-0.0064	0.0392*	0.0064
	(0.0092)	(0.0157)	(0.0120)	(0.0222)	(0.0099)
[C] Private * Post * 1[Rank$_{t-1}$ > Median of Friends in Block]	0.0042	-0.0197	0.0155	-0.0346	-0.0015
	(0.0109)	(0.0212)	(0.0122)	(0.0219)	(0.0122)
[D] Public * Post * 1[Rank$_{t-1}$ > Median of Friends in Block]	-0.0355**	-0.0651***	-0.0142	-0.0489	-0.0399***
	(0.0142)	(0.0216)	(0.0223)	(0.0323)	(0.0146)
[E] Private * Post * 1[Rank$_{t-1}$ > Median of Non-Friends in Block]					0.0157
					(0.0142)
[F] Public * Post * 1[Rank$_{t-1}$ > Median of Non-Friends in Block]					0.0093
					(0.0103)
[G] Post * 1[Rank$_{t-1}$ > Median of Friends of Friends in Block]	0.0274***	0.0309***	0.0224		0.0245***
	(0.0076)	(0.0115)	(0.0146)		(0.0086)
[H] Post * 1[Rank$_{t-1}$ > Median of Non-Friends in Block]					0.0070
					(0.0082)
Observations	13,745	4,654	7,987		13,745
N(Worker)	366	120	216		366
N(Months)	45	45	45		45
Constant	YES	YES	YES		YES
FE: Worker, Year-Month, Style	YES	YES	YES		YES
Other Controls	YES	YES	YES		YES
Social Conformity Effect: (E + H) - (A + D)	-0.0281***	-0.0445**	-0.0149		
	(0.0097)	(0.0184)	(0.0128)		

Note: *1[Rank$_{t-1}$ > Median of Friends in Block]* is a dummy variable that takes the value 1 if a worker was ranked higher than the median rank among his friends (from the same block and the same treatment group) in the previous month. *1[Rank$_{t-1}$ > Median of Non-Friends in Block]* is a dummy variable that takes the value 1 if a worker was ranked, in the previous month, higher than the median rank of all same-treatment and same-block peers that he is not friends with. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. The results show that, relative to Private Treatment, workers in Public Treatment reduced their effort whenever they were ranked higher than their friends.

Indeed, in column 2 we see that publicly ranked workers who were relatively less productive than their friends increased their effort following the introduction of the intervention. Compared to these workers, workers who were relatively more productive decreased their effort by about six p.p. Also, the relatively more productive workers in Public Treatment decrease their effort not only compared to relatively less productive workers in the same treatment, but they decrease their effort also relative to similar workers in the control group. This is shown by the sum of Rows B and D at the bottom of the table.

If this behaviour among relatively more productive workers was induced by making the ranks public, we would not expect to see this in the Private Treatment. Indeed, when we do the same exercise with Private Treatment in column 3, we do not see any such decrease in effort from privately ranked workers who were also relatively more productive than their friends. To interpret this decrease in effort in Public Treatment as social-conformity effect, we should deduct the response by similar workers in Private Treatment. This is shown by the sum of Rows (B+D) - (A+C) at the bottom of the table, which is also negative and statistically significant. Finally, these results are also robust to including additional controls, as evident from column 4.

Table 1.4 shows conformity with respect to baseline productivity. But how did these workers respond to actual rank information? This I test in Table 1.5.

If the social pressure to conform was present, then it would switch on when a worker is ranked higher than his friends. Hence, as a proxy for this social pressure to conform, I use a time-variant dummy variable that takes the value 1 if, in a given month, the worker found out that he was ranked higher than the median of all ranks among the his friends (in the same Treatment) in the previous month.[27]

As before, to ensure that I use appropriate counterfactuals for treated workers, I also assign ranks to control group workers, and compute their rank distances from their friends (in the control group). Treated workers eventually learned their ranks, but the control-group workers did not. Thus, after controlling for counterfactual responses from control-group workers with similar rank distances from their friends, whatever differential effect I pick up on the treated workers is from the information on ranks made known to them. Further, I use worker fixed-effects to pick up within-worker response to monthly variations in ranks; using worker fixed-effects ensures that I do not pick up anything that stems from time-invariant issues, such as a worker's baseline productivity (which is likely to be correlated with the worker's

[27]Notice that using previous month's rank information also allows me to work around Manski's reflection problem (Manski [1993]).

rank). I also use year-month fixed-effects to cancel out any month-specific common shocks, and style fixed-effects to control for variation in wage from style-specific characteristics. Table 1.5 shows the results.

Column 1 of Table 1.5 shows that, on average, a worker in Public Treatment, relative to a similar control-group worker, reduced effort by about 3 p.p. when the worker was ranked higher than the median of his friends in the previous month (sum of rows B and D). Also, to account for intrinsic responses we need to deduct response by similar workers in the Private Treatment. The net effect is shown at the bottom of the table, which is negative and statistically significant. In other words, once a Public Treatment worker found out that he had ranked higher than his friends in the previous month, he reduced his effort by about 2.8 p.p. relative to a Private Treatment worker.[28]

Alternatively, conformity could also be tested using a continuous measure of rank distance with friends instead of a dummy variable as used in column 1 of Table 1.5. Indeed, using a continuous measure of rank distance with friends yields similar results, and shows that the reduction in effort from workers in Public Treatment was more when they are ranked incrementally higher than their friends (i.e. $M_1(.) > 0$), but there were no such effect when they were ranked lower than their friends (i.e. $M(x) = 0$ when $x \leq 0$). These results are omitted for brevity.

To reconcile results from conformity to that from information shocks (as was shown in Table 1.3), in columns 2 and 3, I again split the workers into two subsets based on the feedback they received. We see similar differential responses to feedback (column 4) from Public Treatment as we saw from Private Treatment in Table 1.3.

Regarding conformity, we find that Public Treatment responded more strongly to conformity pressure even when we split workers based on the type of feedback they received, as is shown by the sum of Rows (E + H) - (A + D) at the bottom of the table. Also, the difference is larger in the case where workers received positive feedback.

Finally, what about social status? Now that we have captured conformity through a dummy variable that switches on when a worker ranks above his friends, Public*Post captures the response when he is *not* ranked better than his friends. In other words, the Public*Post coefficient captures response to intrinsic-status motivations (just like in Private*Post) and response to social-status incentives, but not social conformity. Therefore, the difference between Public*Post and Private*Post

[28]Note that the conformity effect with respect to distances from friends' rankings is net of positive status incentives that might also be at work specifically within the network of friends. To that extent, the conformity effect we are capturing is only underestimated.

coefficients is driven by social-status incentives. Returning to column 1 which includes all workers, I find that the effect from social status is positive, but very small and statistically insignificant. In other words, a Public Treatment worker who was not ranked higher than his friends responds very weakly to social-status incentives. Social-status effect is similar in columns 2 and 3 - the differences in Public*Post and Private*Post coefficients are positive but statistically insignificant.

Note that, while Public Treatment workers who were not ranked higher than their friends exert only weakly more effort than similar Private Treatment worker, Public Treatment workers who *were* ranked higher than their friends exert significantly less effort than similar Private Treatment workers. In other words, social-conformity effect outweighs social-status effect for the latter group of workers in the Public Treatment.

Finally, is the negative effect of conformity present only with respect to friends, or do such effects also exist with respect to other reference groups? In column 5 of Table 1.5, I check how the workers responded to getting ranked higher than the median of all the other workers from the same block, who were also in the same treatment, but with whom a worker did *not* socialize outside the factory. The results on conformity to friends still exist (row D), but no such conformity took place with respect to the others (row F).

I repeat the exercise in column 5 by defining the second reference group as either (same block and same treatment) peers of *similar productivity* but with whom a worker did not socialize with[29], or (same block and same treatment) peers with whom a worker *talked inside the factory* but with whom the worker did not socialize outside the factory. The results are similar as in column 5, and hence not reported.

Thus, while the Public Treatment workers did exhibit conformity to their friends, they did not show any such behavior with respect to other workers in their block with whom they were not friends. This is consistent with our hypothesis that a worker would reduce effort to internalize negative externality on peers who can socially punish the worker if he tries to consistently shame them through ranks.[30] Since other workers who are not his friends do not have any strong way to inflict social punishment, the worker does not internalize the negative externality he imposes on them.

[29]For a given worker, a peer is defined as of similar productivity if the peer's pre-intervention productivity was within the 25 percentile band of the worker's own productivity.

[30]Alternatively, they could also do so for altruistic reasons if they felt guilty about their friends ranking worse than them. Although this alternative reason cannot be ruled out entirely, the fact that we do not see any such conformity in Private Treatment indicates that altruism is less likely.

1.6.4 Dynamic Effect

In tables 1.3 and 1.5, I split workers into two subsets based on the type of feedback they received in the first treatment month. In other words, we interpret the treatment effect from all the intervention months as a response to a single piece of information, ranks in the first treatment month. Rankings were also delivered in each of the subsequent treatment months, raising the question of whether the workers respond to subsequent rank information, too.

A worker's response to ranking information would vary across months if he learned something new from the latest information. While the information on true rank in the first feedback letter carried enough new information to update a worker's perceived rank, subsequent information was likely to be similar to that received in the first letter, and, hence, would not carry any new information. In column 1 of Table A.1, I check the correlation between workers' expected ranks as reported during the baseline and the true rank they received in the first month of treatment, February 2016. Column 2, on the other hand, reports the correlation between the rank they received in March 2016 and the rank they received in the previous month, February 2016. These are all cross-sectional regressions with only Private Treatment workers. The correlation coefficients indeed show that while the true ranks in February 2016 were only weakly correlated with the workers' expected ranks, the next rank they received in March 2016 was highly correlated with that from February 2016. Ranks in subsequent months are similarly correlated. Column 3 shows the correlation of ranks between June and July of the year 2016.

To check dynamic responses to this information more rigorously, I also check how workers responded to changes in their ranks across the previous two treatment letters. Table A.2 in the Appendix shows the results. Column 1 considers all workers. Columns 2 and 3 control for the first feedback they received by breaking the workers into subsets based on type of feedback in the first treatment month. In either case, we see little response to changes in their ranks across months.

1.6.5 Further Tests and Discussions

Previously, I interpreted the differential responses of workers to positive and negative feedback as workers re-optimizing their effort once they had learned about the true return to their effort. In particular, we saw that a worker who received a negative feedback in the first treatment month reduced his effort in all subsequent treatment months. Although it makes sense that, upon receiving negative feedback, a worker would become demotivated, would he then not work harder to achieve his perceived rank?

In order to answer the above question, during the baseline survey, we implemented a laboratory-in-the-field game to capture workers' innate competitive nature, thus capturing their willingness to be ranked. In that game, we gave workers 10 ping-pong balls and asked them to throw the balls one at a time into a basket placed 2.5 metres away. We told them that they would be paid for each successful shot they made. However, they could choose to be paid through one of two different methods. The first method would pay them at a fixed piece rate for each successful shot. The second plan would pay them double that rate, but only if the worker scored more than what a randomly chosen peer scored at the same game.[31] We asked the worker to select one of the two payment methods only after he saw the setup of the game, so that he could make an informed decision about which payment he wanted to select. We told the worker that his competitor would be picked only after all the workers had played the game, and only then would we decide who had won. Thus, the worker would not know with whom he would be compared. Among 363 workers surveyed, 198 chose the first version, and 165 chose the competitive version of the game. The numbers of workers choosing one or the other version of the game were also balanced between the experimental arms, as shown in Table 1.1. In the following analysis, I consider the workers who chose the second payment method as having a more competitive attitude than those who did not. If workers cared about their status, workers who were more competitive in nature would perform better than those who were not. Indeed, this is what we see in Table 1.6.

In Table 1.6, Treatment*Post coefficients refer to responses by less- or non-competitive workers, while (Treatment*Post + Treatment*Post*Competitive) tells us the responses of competitive workers. Column 1 shows that competitive workers indeed responded more positively than those who were not competitive. The double interaction terms are positive in both treatments, but statistically significant only for Public Treatment. The next two columns yield cleaner estimates. When split into subsets of workers based on the type of feedback received by the workers, a clearer pattern emerges. The difference between competitive and non-competitive workers is stark for workers who received negative feedback, and is almost identical between the two treatments. Non-competitive workers indeed reduced their effort upon receiving negative feedback. On the other hand, competitive workers did not give up so easily; to the contrary, they marginally increased their effort compared to control group (bottom two rows of the table). Among the workers who received positive feedback,

[31] This game is similar to laboratory or laboratory-in-field games used in existing literature to measure an individual's competitive attitude. For example, see Gneezy et al. [2009].

Table 1.6: Fightback from Competitive Workers

	(1)	(2)	(3)
	Ln(Wage)	Ln(Wage)	Ln(Wage)
		Pos.Feed.	Neg.Feed.
[A] Private * Post	-0.00657	0.0194	-0.0373**
	(0.0100)	(0.0140)	(0.0149)
[B] Private * Post * Competitive	0.0195	0.0109	0.0449**
	(0.0161)	(0.0236)	(0.0210)
[C] Public * Post	-0.0150*	0.00323	-0.0316***
	(0.00846)	(0.0124)	(0.0115)
[D] Public * Post * Competitive	0.0281*	0.00606	0.0423**
	(0.0151)	(0.0274)	(0.0190)
[E] Post * Competitive	-0.0130	0.0228	-0.0410***
	(0.0105)	(0.0169)	(0.0144)
Observations	14,118	4,813	8,287
N(Worker)	363	120	216
Constant	YES	YES	YES
FE: Worker, Year-Month, Style	YES	YES	YES
Other Controls: Block Size	YES	YES	YES
A + B	0.0130	0.0303	0.0076
	(0.0121)	(0.0200)	(0.0140)
C + D	0.0131	0.0093	0.0107
	(0.0108)	(0.0220)	(0.0130)

Note: *Competitive* is a dummy variable that takes the value 1 if a worker chose to get paid through the competitive version of ball-bucket game played during baseline survey. It takes the value 0 if he chose to get paid though uncompetitive piece rate. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. The results show that among treated workers who received negative feedback in the first treatment month, workers who were more competitive in nature fought back and exerted more effort relative to those who were less competitive in nature.

competitive ones increased their effort compared with non-competitive ones, but the differences are not as stark. This would be as expected because, being competitive or not may not make a significant difference when workers find out that they performed better than what they had expected.

The discussion of the differences between Private Treatment and Public Treatment has so far considered only social status and conformity. However, there is another subtle difference between the two treatments. It is not the case that Public Treatment was different to Private Treatment only in that a worker's rank was visible to others. Public rankings also allowed the worker to know about others' true ranks. Hence, there was more information in public ranks than in private. Could having more accurate knowledge of peers' relative ranks drive any of the results we saw in Public Treatment?

To check whether such learning mattered, I make use of another set of information collected during the baseline survey. More specifically, the baseline survey asked each worker in the factory to compare his own wages (from the previous three months) to those of each of the other workers in the same block. To check whether learning about others' ranks mattered, I check how workers in Public Treatment responded to peers' ranks that were different than the wage comparison made by the workers during the baseline survey. Table A.3 in the Appendix shows the results.

To represent new knowledge that was created from knowing peers' ranks in Public Treatment, I use the number of peers whose ranks (in the previous month) were different than a worker's beliefs about his position at baseline. Such peers can be of two types: (a) peers that a worker had thought were comparatively less productive, but who, in fact, ranked higher than him in the previous month, or (b) peers that the worker had thought were comparatively more productive, but who, in fact, were ranked lower than him in the previous month.

For Public Treatment in column 1 of Table A.3, the only information shock that seemed to matter surfaced when workers discovered that, contrary to their expectations, their peers ranked lower than themselves. To check how much of this is driven by conformity to friends, in column 2, as before, I include a dummy to indicate whether or not a worker was ranked higher than the median of the worker's friends. We see that conformity still exists, and clearly was not driven by simply knowing more about other peers' productivity. Nonetheless, peers receiving unexpected relative ranks seemed to matter, but mostly for workers who received negative feedback.

Conversely, for Private Treatment in column 3, we see absolutely no impact of such specific information shocks; both the relevant coefficients are very small in size. This is, of course, what we would expect because the Private Treatment workers never received information about their peers' ranks.

1.7 Alternative Explanations

One concern in the experimental design of this paper is whether the private ranking treatment indeed remained private, or whether the Private Treatment workers instead shared their rankings with others - effectively making the treatment public. Note that, if Private Treatment workers did indeed share their rankings with others, we would not expect to see any differential responses between Private and

Public Treatment workers when they were more highly ranked than their friends (as shown in Table 1.5). The difference was particularly big and statistically significant when workers received positive feedback from treatment letters. This is inconsistent with information sharing because we might expect them to share their rankings particularly when they receive good news than when they receive bad news. Finally, in Table A.3 column 3, we see that Public Treatment workers responded to new knowledge about peers' productivity, but the Private Treatment workers showed no such response; if the ranking information were being shared, we would expect to see Private Treatment workers responding to new information in a way similar to the responses of the Public Treatment workers.

Could the fact that Public Treatment workers reduced their effort when they were ranked higher than their friends be explained by complacency? Note that, in column 5 of Table 1.5, we found that a Public Treatment worker reduced effort when he was ranked higher than his friends but the worker did not reduce effort when he was ranked higher than peers with whom he did not socialize. If what we interpreted as social-conformity effect was in fact driven by complacency, we would expect to see similar reduction in effort even with respect to peers with whom a worker did not socialize.

Column 5 of Table 1.5 rules out complacency among Public Treatment workers, but what about in Private Treatment? Again, this is unlikely. In the Private Treatment, we would expect a worker to be complacent when he receives a higher ranking than he had expected (positive feedback). Recall that in Observation 1 we had that conditional on $H_{11}(.) < 0$, workers decrease (increase) effort when they receive positive (negative) feedback. This, in fact, can be interpreted as complacency effect. But instead, the empirical evidence suggests the contrary: workers increased their effort when given positive feedback, and hence we found empirical support for $H_{11} > 0$ instead.

Could any of the positive effect among workers be explained by fear of getting fired? Note that we saw an increase in productivity only when workers received positive feedback, while we saw a decrease in productivity when workers received negative feedback. If any response were driven by fear of getting fired, we would expect to see the opposite reaction. Moreover, the treatment letters consistently reminded workers that the rankings would not have any effect on their jobs.

Finally, following the start of the experiment, could there have been a redistribution of sweater styles among workers that might explain the results? This should not be a concern for us because we have almost always controlled for style fixed effects. Nonetheless, if we compare the results with or without the style fixed effects (results omitted for brevity), they remain the same.

1.8 Conclusion

Existing literature suggests that status incentives, in the form of performance-based ranks, can increase worker productivity. However, the evidence in this paper indicates at least two reasons why this may not always be the case. A novel experimental design with private and public ranking, along with detailed baseline data on workers' expected ranks and their social network, help to show that demotivation and social conformity can strongly counteract the positive effects of status motivation.

In particular, the evidence found in this paper indicates that if ranked privately, demotivational effects are likely to offset at least some of the positive effects of intrinsic-status incentives from ranks. If ranked publicly, workers' preferences to socially conform with their friends can lead to even worse results by offsetting the weak positive effect from the additional social-status motivations. Nonetheless, the results from this paper also suggest that rank-based incentives are more likely to increase productivity on average if workers in a given context are highly competitive in nature. Such competitive attitudes will offset the negative demotivational effect from negative feedback, and, in turn, will complement the positive effect from positive feedback. Similarly, social conformity effects will be diminished if there are thinner social connections at a given workplace.

It is worth pointing out that the experiment in this paper was conducted in a developing country. While intrinsic motivational and demotivational effects are likely to be the same in developed and developing countries, social conformity, arguably, may be particularly strong in a developing-country context. Because of limited access to financial institutions, social capital may play a bigger role in a developing country to help workers cope with short-term shocks to income. This, in turn, makes social capital much more valuable in such a country. Hence, it is possible that workers in developing countries respond to social-conformity incentives more strongly than workers in a developed country. However, we do not have concrete evidence on this in the existing literature; this may be an avenue for future research.

Chapter 2

Firing, Relational Contracts, and Productivity in a Bangladeshi Sweater Factory

JOINT WITH: ROBERT AKERLOF, ROCCO MACCHIAVELLO, ATONU RABBANI AND CHRISTOPHER WOODRUFF[1]

2.1 Introduction

Conflict between management and production workers is almost unavoidable in most organisations (see, e.g., Dahrendorf [1959]). Appropriate management of this conflict assumes particular relevance in fast-growing developing countries in which poorer working conditions and rapid transformation exacerbate industrial relations as parties struggle to adapt expectations to changing circumstances. Indeed, reports of labour unrest, strikes, and lockouts are common in emerging industrial powerhouses such as Bangladesh, China, Ethiopia, and Vietnam.[2]

These episodes of conflict pose significant challenges to a firm's management: by destabilising the subtle web of relational arrangements in the workplace, temporary episodes of conflict might threaten long-run productivity (see, e.g., Gibbons and Henderson [2012], Jin and Matouschek [2013]). Empirical studies of how labour

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[2]For example, on China see issues of The Economist on January 31, 2015; April 26, 2014; and January 28, 2012. For Bangladesh, see, e.g., The Economist on February 7, 2015. For Vietnam, see The Economist on June 7, 2014.

unrest affects workers' and firms' productivity are, however, scarce. We conjecture that this paucity is likely to be ascribed to the formidable empirical challenges of obtaining information on productivity before, during, and after episodes of labour unrest - rather than to the irrelevance of the subject at hand.[3]

In this paper we take advantage of continued access to the production floor (and internal records) of a large Bangladeshi sweater factory. Two aspects of the environment are particularly noteworthy: first, we observe this workplace during a period characterised by intense episodes of labour unrest that culminated in the firing of approximately one-quarter of the relevant work force. Second, we focus on the manual knitting section, in which workers individually perform tasks using standardised capital and material. This allows us to obtain precise measures of individual workers' productivity before, during, and after the unrest. We combine this information with uniquely detailed qualitative observations of this workplace, and with workers' surveys undertaken around the time of the unrest - providing a unique window into how production relations were affected by the unrest.

We focus on how exposure to the firing affected survivors' productivity - a heavily debated issue in the organisational literature (see, e.g., Brockner [1988], Cascio [1993], Mishra and Spreitzer [1998]), and a subject about which economic theory yields ambiguous predictions. On the one hand, layoffs might have a negative effect on survivors' productivity. For example, workers might become discouraged, perceive that they face greater uncertainty, and/or grow angry at management. On the other hand, layoffs might induce positive productivity responses among survivors: there is weaker competition for promotion; the most disruptive workers have been removed; and fear of dismissal increases incentives. Given that these different mechanisms appear to be a priori equally likely, establishing a coherent set of results in either direction - albeit in a specific setting - appears to be valuable.

Across a variety of empirical specifications, we find a negative impact of exposure to the firings on survivors' productivity. Evidence from our analysis suggests that, in general, there is a drop in productivity of at least 0.03 standard deviations for each peer fired from a worker's own group. The drop nearly doubles in magnitude if the peer worked in a very close proximity or in line of sight. We do not find any effect from a fired peer from a different group, even if he was working in very close proximity.

Our findings resonate the qualitative observations we made on the factory

[3]Such evidence would, for instance, deepen our understanding of the determinants of productivity dispersion across firms (see, e.g., Hsieh and Klenow [2009] and Asker et al. [2014]), and complement existing evidence on the importance of formal management practices and processes in determining productivity (see, e.g., Bloom et al. [2012]).

floor, where we find that workers who are located next to one another are more likely to talk to each other on a frequent basis (e.g. share jokes or small complaints, etc.), and are also likely to help each other with quick tips or even physical assistance. We have also detected a strong sense of group identity that exists along the line of administrative groups formed for better supervision. Based on our quantitative analysis, and supported by these qualitative observations, we conclude that workers are adversely affected by loss of peers with whom they were more likely to interact on a day-to-day basis, and with whom they could more frequently associate. This could be driven by the inability of these workers to come to terms with the changed environment, and the remaining workers' upset about the firings.

While we have found strong associations between productivity and spatial locations of fired peers, we do not find any such association from any other forms of proximity that could also form the basis of relational intensity. For instance, we have tested the assumptions that people may be more likely to be friends with peers of similar ages or of similar skill levels. We do not find evidence of any impact generated by these alternative notions of proximity. We are also able to rule out alternative hypotheses for the reduction in productivity, such as: the possibility that factories may target and punish some of the surviving workers even after the firing episode; or the possibility that the firing of a peer led to a lack of productive interaction (such as learning); or that the surviving operators spent more time helping the newly hired operators to settle down.

The paper is organised as follow: Section 2.2 discusses the existing related literature. Section 2.3 provides a short background and context of the workplace and incidents. Section 2.4 briefly discusses the data that we use. Section 2.5 discusses the empirical approach that we take, and Section 2.6 presents the results. In Section 2.7, we conclude.

2.2 Related Literature

This paper contributes to different strands of the literature. Several papers have found that labour unrest can lead to fall in product quality (see, Krueger and Mas [2004], Mas [2008], Katz et al. [1983], and Kleiner et al. [2002]). There are three main differences between these papers and ours. First, we study an event involving a substantial amount of worker lay-offs. In contrast, these papers focus on instances of strikes. Second, we can directly focus on individual workers' productivity levels - rather than more aggregated, firm-level outcomes. Finally, we complement internal administrative records from the factory with qualitative observational evidence and with worker surveys.[4]

Hjort [2014] finds (ethnic) conflict outside the workplace leads to reduced cooperation along ethnic lines, and a consequent reduction in firms' overall productivity. Our paper, however, looks into conflict between workers and management, rather than between workers.

This paper is also related to the literature on peer effects, such as Bandiera et al. [2005] and Mas and Moretti [2009]. The first paper finds that workers reduce their productivity when they work with friends in a setting where there are negative externalities of being productive; the second finds a positive spillover effect of highly productive peers. But again, these are 'contemporary spillover effects' in the sense that the peers work together at the same time at the same place. What we look for, instead, is how peers affect each other once they have parted because of decisions made by the management.

Casaburi and Macchiavello [2015] study a Kenyan dairy cooperative that tries to re-enhance loyalty among members by threatening to expel non-complying members (the equivalent of lay-offs in our context). They find that such stern threats by management have mixed results (some farmers increase their cooperation, while others leave the cooperative) and are hard to enforce in practice.

From a methodological point of view, this study has benefited from continuous presence on the production floor of one or two members of our survey team. Our approach, which follows in the spirit of Homans' pioneering study on social relations in the workplace (Homans [1950]), has enabled us to obtain not only a detailed understanding of production processes but also (and, perhaps, more crucially) of the fabric of social relations within and across layers of the managerial hierarchy.[5] We have also been able to complement the administrative records and floor observations with various surveys and games that help us obtain measures of trust and respect within the factory, among workers, and between workers and management.

2.3 Background

To answer the research question we pose in this paper, we use a series of incidents of labour unrest that eventually led to a large-scale firing/lay-off episode at a large,

[4]A prominent but mostly qualitative literature in management sciences studies the effects of workers' lay-offs on surviving co-workers. For instance, Brockner et al. [1987] suggest that workers who most closely identify themselves with fired workers, and who think that the lay-off was unfair are the most negatively affected (see also Brockner et al. [1993] and Brockner and Wiesenfeld [1993]). We are not aware of quantitative studies in this literature.

[5]Fischer and Karlan [2015] provide an example of qualitative surveys in the context of understanding constraints to growth among micro-enterprises.

vertically integrated sweater factory in Bangladesh. The main compound of the factory is vertically interlinked from Yarn Winding section that prepares yarn for knitting, to completed Sweater Packaging sections that make the sweaters ready for export.

Knitting is carried out in three different subsections of the factory: Manual Knitting, Semi-Automatic Knitting, and Automatic Knitting. Each subsection usually works on different styles of sweaters or on specific parts of a given style. This paper focuses exclusively on the Manual Knitting Section, in which almost all the workers are males. In this section, workers, called *operators*, use knitting machines to manually knit yarn into sweater parts (typically one to four parts, depending on the styles) that are later passed on to the Linking Section to have all the parts linked together to form the sweater. A picture showing an operator knitting a sweater in the Manual Knitting Section, and another picture showing knitted parts are provided in Figure B.1 and B.2 in the Appendix, respectively.

The Manual Knitting Section of this factory employs about 400 operators (this changes over time but most of the time the section has 400 or more operators), grouped into *blocks* of about 30 workers, with a supervisor dedicated to each block. The number of blocks changed during the period of this study, but by the end of the period we use in this paper, there were 15 blocks on the floor. The 15 supervisors are, in turn, supervised by one Floor-In-Charge (FIC), who, again, is supervised by the Production Manager. In a given month, the section works on multiple orders, leading to simultaneous production of multiple styles. On a given day, an operator available for a new job is assigned to produce a batch of 12 sweaters of a particular style; completion of that job may take anywhere from a few hours to more than a day depending on the complexity of the style. This allocation of styles is done by a few *distributors* from the Distribution Section within the Manual Knitting Section, in consultation with the FIC. The full organogram of the Manual Knitting Section is provided in Figure B.5 in the appendix. The operators are paid monthly on piece rates, with the rates varying across styles, and determined by management.

2.3.1 Timeline of Events

We follow individual-level production of all the operators from the Manual Knitting Section over the period from January 2013 to August 2014. A graph showing detailed timeline of the key events affecting this section during this period is given in Figure B.3 in the appendix.

Over the first seven months of this period there were no reports of any labour unrest in the section. In August 2013, however, the Manual Knitting Section was hit by protests and unrest by operators, who were reportedly unhappy about the general level of piece rates they were being paid. The Section had to be temporarily closed down. It was reopened a few days later once the operators and the management came to an understanding. In February 2014, the management decided to isolate the Manual Knitting Section from the rest of the compound, and to move it to a new location about a mile away. The operators were unhappy about the move, and they protested again. This led to the section closing again for another 17 days. But almost all the operators returned when the section re-opened. Finally, in the first week of April 2014, the operators once again staged another protest against perceived 'low' piece rates. This time the protest turned more aggressive and violent. At one stage, the FIC was physically harassed and injured by some operators. However, not all operators were at the forefront of the protest; some were more vocal and aggressive than others. Again, the new compound was closed down. For alleged involvement in the violence, 102 of the 407 operators were fired. Law suits were filed against many of these fired operators. Six supervisors were also fired, allegedly due to their role in the unrest. The factory was finally reopened in mid-May, after a little more than a month idle. New operators were hired to replace the fired workers. Prior to the labour unrest, we had started a survey to understand the social network on the floor; this particular protest and closure of the factory came at a point when we had completed surveys of the supervisors only, and were about to start the operator survey. Hence, we were not able to survey the pre-firing social network among the operators.

For this paper we focus only on the Manual Knitting section. All the workers in the Manual Knitting Section work on directly comparable products, using the same capital, inputs, and technology. We also observe many details that can help us estimate their productivity once such other elements are accounted for. This helps us to get very precise measures of productivity, which would not be possible if we included different sections with different outputs and technologies in our sample. Also, because the Manual Knitting Section was the most affected by protests and firings, this makes it a uniquely special case of interest.

2.4 Data

We obtained detailed monthly production data for all operators in the Manual Knitting Section for the period from January 2013 to August 2014 (20 months). The data contain information on which styles of sweaters an operator worked on in a given month, the sizes of the sweaters, how many of each he produced, and details of the final payment made to him. These data are matched to administrative records for each operator, including tenure at the factory, age, attendance records, and, for operators who no longer work at the factory, date of quitting or firing.

The internal production and administrative records at the factory are combined with two additional sources of information. First, we implemented short surveys of both supervisors and operators. As previously mentioned, we were able to complete a survey for supervisors of all the blocks (14 supervisors for 15 blocks at that time; one more supervisor was hired later) in March 2014, which was before the unrest that led to the firing in April 2014. This survey helped to identify the social network among the supervisors, their respect for each other, perceived skills of others, etc. Later in the year, from October to December 2014, we conducted another survey for operators. We surveyed 279 operators, including both surviving and newly hired operators. This survey asked about their perception of fairness in different aspects of production such as piece rates, distribution, quality inspection, etc. It also included a series of trust games with real money pay offs that can help us to determine the level of trust they have in peer operators, or with different hierarchies of the management, including distributors and quality inspectors.

Second, members of our observation team visited the factory at least three days a week from January 2014, till the end of the sample period considered in this paper. During these visits, the two team members developed relationships with all the operators, supervisors, and other workers on the floor. We observed the working processes, working environment, and the behaviours of operators and other workers on the floor from a close distance on a regular basis, while, simultaneously, trying to ensure that our presence did not alter any of the ongoing systems or behaviours. These survey data and observations made on the floor are highly informative about the kinds of social networks that exist on the floor; both the data and observations also play a key role in driving the analysis that we do later.

Table 2.1 below shows some key descriptive statistics about the floor and the firing incidence. The number of operators and blocks varied across time; so, we present the statistics at the point of firing. Information that emerges from the second part of the table shows that: 1) the average daily wage for fired workers is lower than that of the surviving workers, and 2) tenure in months calculated in March 2014, is higher for fired workers than for survivors. Records for some operators in our dataset do not contain the month of joining, which is why we cannot compute their tenure (as evident by a relatively lower n for tenure).

Table 2.1: Key Descriptive Statistics for Production Data

	Obs.	Mean	Std. Dev.	Min	Max
Age of Fired Operators	84	338.33	42.33	267.00	431.00
Age of Survivors	287	338.70	52.33	258.00	526.00
Tenure (Months) in Mar'14: Fired Operators	85	67.04	18.34	34.70	106.03
Tenure (Months) in Mar'14: Survivors	304	63.39	19.13	25.77	121.67
Avg. Daily Wage of Fired Operators	1614	361.69	113.60	29.14	1163.56
Avg. Daily Wage of Survivors	5920	390.58	113.86	34.29	1172.90
# of Op. in Block in Mar'14	305	28.17	3.09	9	31
# of Op. Fired in Block in Apr'14	305	6.49	3.31	2	14
# of Op.: Circle 1 & Same Block (Mar'14)	303	5.00	1.71	1	8
# of Op. Fired: Circle 1 & Same Block (Apr'14)	303	1.15	1.21	0	6

Note: The first half of this table compares some of the basic characteristics of fired operators and surviving operators in the manual knitting section. The second half shows descriptive statistics on the number of operators fired from blocks and immediate vicinity of surviving operators, but within the same block. *Circle 1* refers to the pool of a maximum of eight workers surrounding a surviving operator. The fired operators seem to have been at the factory longer but earning a lower average daily wage than the surviving operators.

2.5 Empirical Approach

Our whole empirical approach is divided into two steps. In the first step we estimate the productivity of each operator. In the second step, we look at the impact of firing of peers on the productivity of the surviving operators, conditional on their exposure to firing and prior productivity. This section describes the key points in each of these steps in detail.

2.5.1 Estimating Productivity

Because the operators in the factory are paid on piece rates, one potential candidate for measurement of productivity could have been the wages earned by each operator. However, because the styles allocated to operators are different across operators and time, characteristics of different styles (including their complexity and piece rates) can also determine each operator's output, and, in turn, the payment they receive. Similarly, to the extent that there is a seasonality component in sweater production, leading to some busy and some relaxed months, and to the extent that this affects the speed of production and associated payments in different months, we need to account for the seasonality component as well.

Hence, we estimate the productivity of operators using fixed-effects regressions, while controlling for styles and seasonality. As discussed shortly, we estimate this productivity for three different time periods. Productivity estimated by these fixed effects can be interpreted as the part of an operator's wage that comes from of a combination of time-invariant innate skills/talent of an operator, and the average effort that he exerts in a given period. We attribute any variation we may find in productivity of an operator across time to variation in the average level of effort that he exerts in these different periods, while his innate skills/talent remain relatively unchanged.

The empirical model that we estimate for each period is the following:

$$W_{im} = \alpha + \theta_i + \sum_{s=1}^{\bar{s}} (\tau_s * \rho_{ims} * d_{ims}) + \mu_m + \epsilon_{im} (2.1)$$

where, W_{im} is the average daily wage earned by operator i in month m. θ_i is the operator's fixed-effect, τ_s is the fixed-effect of style s, which is weighted by ρ_{ims} - the share of total working time in month m that operator i spent on style s. d_{ims} is a dummy variable that takes the value 1 if operator i worked on style s in month m. $\bar{s}$ is the total number of styles available in the dataset. μ_m is year-month fixed effect of month m.

Unfortunately, we do not have the data to compute a direct measure of ρ_{ims}, which is why for this version of the analysis we proxy it by the share of wages that operator i earns from style s in month m. One concern with this formulation might be this leading to W_{im} on both sides of the equation. Under the assumptions that: (1) style assignment is random (which rules out favouritism and possible strategic considerations on the side of operators), and (2) production function is super-modular in 'theta' and 'complexity', the estimated theta should be increasing transformations of the underlying theta, and, hence, our estimates of theta would be able to give us a cardinal ranking of the operators' productivity.

For each operator, we estimate productivity for three different time periods: (1) January 2013 - June 2013, when there was no labour unrest; (2) September 2013 - March 2014, during which the three instances of labour unrest took place; and (3) June 2014 - August 2014, which is the post-firing period. In the rest of the paper we refer to these three periods as Periods 1, 2, and 3 respectively. We remove the July 2013 - August 2013 time frame from the sample because the first unrest happened in August 2013, and including this period in the sample may 'pollute' our estimates of productivity. For similar reasons, we also leave out the April 2014 - May 2014 time frame when the last violent unrest took place, eventually leading to the firing episode.

Once the above model is estimated for each of the three periods, we save the individual fixed effects θ_i as measures of productivity for a given period. Period 1 estimates serve as a benchmark in the sense that we can use them to compare how productivity (that is, the effort component) changed in the relatively unstable period (Period 2) and in the post-firing phase (Period 3).

2.5.2 Post-firing Productivity

In this part, we want to check how the productivity of the surviving operators changed in the post-firing period. In different variations, we estimate the following core model:

$$\theta_i^3 = \alpha + \beta_1\theta_i + \Phi_i^{'}\beta_4 + X_i^{'}\beta_5 + \eta_i \tag{2.2}$$

θ_i^3 is the productivity of operator i in the post-firing period (Period 3), and θ_i is his productivity in pre-firing period (primarily to control for the persistence in the operator's productivity). Our key interest is in Φ_i, which is a set of various measures of operator i's exposure to the firing; X_i is a set of other controls.

To understand how we measure exposure to firing, it may be useful to first look at some observations that we did to understand the dynamics within the network of operators. In Table B.1 and B.2 in the appendix, we present a couple of sample observations recorded by the research team while observing some operators at work. These observations describe the activities of an operator in great detail, including the clock times for each. We find a lot of interactions between workers, even while they are working. These interactions range from sharing a joke out loud to walking to another operator's workstation and having a chat. We also find that operators help each other with their work, either physically or by offering advice. Finally, these interactions seem to be more common with operators who are in the immediate neighbourhood and, especially, with fellow operators in the line of sight.

Motivated by this, we explore exposure to firing mainly in two different dimensions: (a) same block versus other blocks, because the block-identity generates a sense of belonging to a common group; and (b) alternative notions of relational distance such as spatial distance, age distance, and productivity distance - to a fired worker.

We measure spatial distance by two alternative methods. For the first, exposure of firing for an operator i is given by:

$$E_i = \sum_{j=1}^{n}\left(\frac{F_j}{D_{ij}}\right) \tag{2.3}$$

where n is the total number of co-workers in the same block when measuring exposure to firing within block, or total number of co-workers in other blocks when measuring exposure to firing of other co-workers. F_j is a binary variable taking value

1 if operator j is fired and zero otherwise, and D_{ij} is the Euclidean distance between operators i and j. Thus, a fired peer is given a higher weight if he is spatially closer, while being distant earns him a lower weight. This is intended to reflect the fact that operators interact more with peers located closer, and firing of these peers should count more than firing of others.

To investigate possible non-linearity in the effect of spatial distance, we develop an alternate measure of spatial distance in which we break an operator's peer group into circles radiating out from his work station. Each peer in a given circle is the same *worker distance* away from the worker in the centre. The first circle contains operators who are immediate neighbours. As a result, this circle can contain a maximum of eight operators: three at the front, three at the back, one on left, and one on right. This can be seen from the floor map in Figure B.4 in Appendix, where each box represents one operator. Each line of operators face the other line in front of them while they put their back to the line at their back (this is because each of the machines has two faces, with one operator working on each face). The next circle contains 16 operators, and so on. We then use the extent of firing in each of these circles as a measure of exposure to firing for the survivors. If an operator were affected by firing of his peers, we expect for this effect to be stronger as the result of firing of those in closer circles than firing of those in the more distant ones, because the 'inner circle' peer group would play a stronger role in reminding him of the firing incident.

In essence, what we are trying to capture by the spatial distance is the intensity of relationships between two operators on the floor. We recognise that the intensity may not necessarily have to be based on spatial proximity; it can also be a function of how close in age two operators are. So as an alternate measure, for operator i, we compute mean age distance with fired operators as:

$$\text{Age Distance}_i = \sum_{j=1}^{n} \Big(\frac{F_j |Age_i - Age_j|}{(Age_i - Age_j)/2} \Big) (2.4)$$

where F_j and n have the same definitions as in Equation 2.3 and Age_k is age of operator k in months in March 2014 (i.e. the month immediately before the firing episode). This measure is to reflect the fact that operators who are closer to each other in terms of age are more likely to be friends. Along the same line of thinking, it can be argued that perhaps workers in the same tiers of productivity are more likely to be friends; hence, we also compute productivity distance, given by:

$$\text{Theta Distance}_{ij} = \sum_{j=1}^{n} \Big(F_j |\theta_i - \theta_j| \Big) (2.5)$$

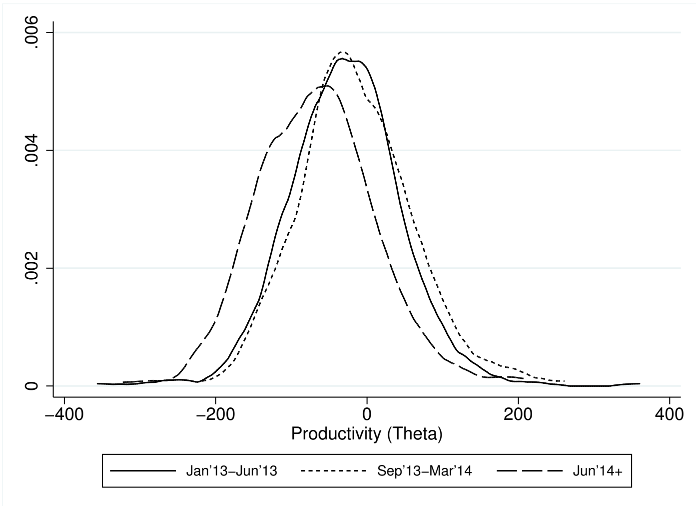
Again, F_j and n have the same definitions as in Equation 2.3, and θ_k is pre-firing productivity for operator k.

The next section presents results for the above estimations.

2.6 Empirical Results

We start with estimation of Equation 2.1. As omitted categories in Equation 2.1 we drop one operator and one style common in all the three periods. This enables us to directly compare the fixed effects across periods. Figure 2.1 shows the distribution of productivity of all operators on the floor in each of the three periods. Test of variances cannot reject the null hypothesis that the variances are same for any given pair of distributions. While the two pre-firing productivity distributions do not seem to differ from each other, we do observe a leftward shift in the distribution for post-firing productivity.

Figure 2.1: Productivity Dispersion in Three Periods

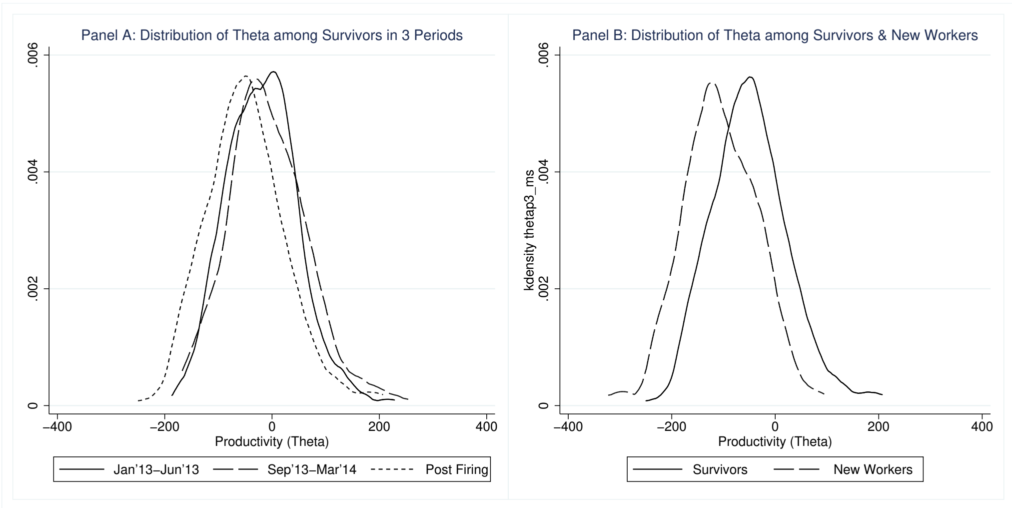


Note: This figure plots distribution of worker fixed effects estimated from Equation 2.1 separately for three different periods - two pre-firing periods (a) January 2013 - June 2013, and (b) September 2013 - March 2014, and one post-firing period June 2014 - August 2014. It includes all the workers available on floor during these periods.While the two pre-firing distributions are very close to each other, we find a leftward shift in the distribution for post-firing productivity.

Indeed, semi-parametric and parametric estimations suggest that operators with relatively low productivity rates who had been employed at the factory for longer were more likely to have been fired during the unrest. Detailed results are reported in Figure B.6 and Table B.3 in the appendix.

Figure 2.2 shows the productivity of survivors (Panel A) and how it compares to the productivity of the newly hired operators (Panel B). Statistically, the variance in productivity of survivors increases in the second period and remains that high in the post-firing period. As hinted in Figure 2.1, we do see that the productivity of survivors has shifted left in the post-firing period. This could not have been driven by any seasonality component because we have derived these measures of productivity after controlling for year-month fixed-effects. Their productivity also seems to be higher on average than that of the new workers.

Figure 2.2: Productivity Dispersion for Survivors and New Workers



Note: Panel A plots distribution of worker fixed effects for only the surviving operators in the three different periods - two pre-firing and one post-firing. It shows a leftward shift of productivity in the post-firing period. Panel B compares the distribution of surviving operators' productivity with that of the newly hired operators.

Finally, we estimate Equation 2.2. Table 2.2 below shows how the survivors' post-firing productivity is correlated with their exposure to firing within their blocks. Measures of productivity, on both sides of the regressions, are standardised.

We use the number of operators fired in the block as a measure of their exposure to firing. We would ideally want to control for the number of operators in the block prior to the firing as well; however, we should also be wary of the fact that the two numbers could potentially be slightly correlated. Hence in column 1, we regress post-firing productivity on the number of operators fired, while in column 2 we additionally control for the number of operators prior to the firing. Largely similar coefficients in the two specifications are reassuring, showing no evidence of a possible multicollinearity problem. The coefficient shows that there is a drop of 0.03 standard deviations in post-firing productivity with each additional peer fired from the block.

Table 2.2: Post-Firing Changes in Productivity for Individual Workers

	(1) P3 Prod.	(2) P3 Prod.	(3) P3 Prod.	(4) P3 Prod.
	Theta=P1 Prod.	Theta=P1 Prod.	Theta=P2 Prod.	Theta=P2 Prod.
# Fired in Block	-0.0345**	-0.0362*	-0.0320**	-0.0332*
	(0.0154)	(0.0215)	(0.0130)	(0.0179)
# of Op. in Block		0.00580	-0.00509	-0.00484
		(0.0350)	(0.0225)	(0.0267)
Standardized Theta	0.793***	0.794***	0.846***	0.837***
	(0.0421)	(0.0500)	(0.0374)	(0.0380)
1(Supervisor Fired)				-0.0483
				(0.0949)
Constant	0.204	0.0515	0.322	0.0773
	(0.133)	(0.910)	(0.637)	(1.048)
Observations	295	295	295	278
Adj. R-Squared	0.679	0.678	0.760	0.756
Age & Tenure Controls	NO	NO	NO	YES

Note: Productivity estimates on both left hand side (LHS) and right hand side (RHS) are standardized. Columns 1 and 2 use Period 1 productivity estimates on RHS while columns 3 and 4 use Period 2 productivity estimates. *1(Supervisor Fired)* is a dummy variable to indicate whether the supervisor of an operator's block was also fired in April 2014. Age (in months) controls are used in a quadratic functional form and tenure (in months in March 2014) enters linearly. Standard errors are bootstrapped and clustered at block level. *, **, *** indicate significance at 10%, 5%, and 1% respectively. The results show that post-firing productivity drops with each additional peer fired from block.

In columns 1 and 2 we use Period 1 productivity as a measure of pre-firing productivity while in column 3 we use Period 2 productivity. Adjusted R-square increases and we also find that the pre-firing productivity explains post-firing productivity slightly better. Hence, in all the regressions to follow, we use Period 2 productivity as a measure of pre-firing productivity.

As mentioned in an earlier section, supervisors of some of the blocks were also fired; working under new supervisors could also be driving some of these results. To address this concern, in column 4, we control for whether a worker was working under a new supervisor in the post-firing period. We also control for a few more worker-specific characteristics, such as age and tenure controls, because earlier evidence suggested that firing was correlated with age and tenure. The coefficient of our interest does not change much. High persistence in performance across periods is evident as a one-unit change in the standard deviation of pre-firing productivity is associated with at least a 0.8-unit change in the standard deviation of post-firing productivity in all the specifications.

Table 2.2 serves to show that there is a negative and robust correlation between the lay-offs of co-workers and survivors' post-firing productivity. It is robust to different measures of past productivity, workers' controls, as well as whether a worker's supervisor was fired. Nonetheless, it is still not clear how firing in other blocks might have affected the survivors. We cannot include the number of operators fired in other blocks in the same regressions, however, since it then becomes perfectly collinear with the number of operators fired in the same block (sum of the two is constant for all operators). More importantly, since there are only 15 blocks, we do not get a big variation in exposure to firing by using block-level firing measure.

To compare the effect of firings within a worker's own block versus the effect of firings in other blocks, and to dig deeper into the mechanism, we use individual-level variation in exposure to firing in Table 2.3, where we introduce alternative measures of exposure to firing as discussed in the previous section.

In column 1 of Table 2.3, exposure to firing is measured by spatially weighted exposure to firing computed from Equation 2.3. To understand the variable of weighted exposure it may be useful to realize that losing one additional peer from the immediate neighbourhood is equivalent to an increase of 0.7 unit of the weighted exposure value; in other words, a one-unit increase in weighted exposure to firing is equivalent to losing 1.4 operators from the immediate neighbourhood (Circle 1, as discussed previously). Similarly, a one-unit increase in weighted exposure to firing is equivalent to losing 2.8 operators from Circle 2, 4.2 operators from Circle 3, so on. We compute this measure separately with respect to peers fired within one's own block and peers fired outside his block. We find a staggering contrast between the two coefficients. Losing peers within one's own block is negatively associated with post-firing productivity of survivors, while losing peers outside one's own block does not have any effect at all; if anything, the effect is marginally positive but not statistically significant.

We next address the question of whether stronger links among workers of similar age play a role. Instead of using spatial distances, we use the mean of age distances with fired operators computed by Equation 2.4. Results are shown in column 2. We do not seem to pick up any effect from age proximity. Alternatively, we might expect that workers befriend co-workers of similar skills, ambitions, etc. To address these issues, we use the measure of productivity distances as given by Equation 2.5. Results are shown in column 3. Again, the proximity of productivity with one's fired peers within one's own block does not seem to have any statistically significant association with post-firing productivity of surviving workers.

In column 4 we combine all the alternative measures, while in column 5

Table 2.3: Post-Firing Changes in Productivity - Different Measures of Proximity

	(1) P3 Prod.	(2) P3 Prod.	(3) P3 Prod.	(4) P3 Prod.	(5) P3 Prod.
Std. Period 2 Theta	0.859***	0.868***	0.821***	0.800***	0.796***
	(0.0393)	(0.0288)	(0.0478)	(0.0600)	(0.0595)
Std. Weighted Exposure to Firing: Same Block	-0.0912**			-0.101**	-0.0934**
	(0.0449)			(0.0431)	(0.0438)
Std. Weighted Exposure to Firing: Other Blocks	0.0155			0.0107	0.0129
	(0.0483)			(0.0454)	(0.0459)
# of Op. in Block	-0.00912			-0.00986	-0.00877
	(0.0226)			(0.0275)	(0.0276)
Std. Mean Age Distance with Fired: Same Block		-0.0259		-0.0320	-0.0337
		(0.0333)		(0.0501)	(0.0597)
Std. Mean Age Distance with Fired: Other Blocks		-0.0626		-0.0592	-0.114
		(0.0465)		(0.0421)	(0.0823)
Std. Mean Theta Distance with Fired: Same Block			-0.0259	0.0179	0.0111
			(0.0594)	(0.0679)	(0.0618)
Std. Mean Theta Distance with Fired: Other Blocks			0.128**	0.0865	0.0962
			(0.0590)	(0.0732)	(0.0683)
Constant	0.228	-0.0249	-0.0257	0.253	2.108
	(0.653)	(0.0478)	(0.0461)	(0.791)	(1.420)
Observations	295	278	295	278	278
Adj. R-Squared	0.758	0.745	0.758	0.762	0.761
Other Controls	NO	NO	NO	NO	YES

Note: *Weighted Exposure* is spatially weighted exposure calculated from Equation 2.3 in the paper. *Age Distance* and *Theta Distance* are calculated from Equation 2.4 and 2.5 respectively. Estimates of productivity on LHS are standardized. On RHS, *Std.* implies standardized. Age (with quadratic specification), tenure and a dummy of whether supervisor was fired constitute the other controls whenever used. Standard errors are bootstrapped and clustered at block level. *, **, *** indicate significance at 10%, 5%, and 1% respectively. The table shows that post-firing productivity is negatively correlated with spatially weighted exposure to firing but is not statistically associated with alternative measures of relational distances.

we additionally control for other controls such as age, tenure, and firing of supervisor. The negative correlation between spatially weighted exposure to firing is robust to these specifications, despite including alternate measures of exposure; the result stands to suggest that the negative association between peers fired and post-firing productivity is actually driven by spatial distance between those fired and the survivors. This is consistent with the ethnographic evidence collected in the field. Colleagues who worked near one another were more likely to have developed friendships. Even among the peer group of proximate colleagues, only those workers who were in the same block as the survivors mattered in terms of playing a role on post-firing productivity.

There does seem to be a small and relatively unstable effect of firing of co-workers of similar productivity levels. It is possible that such an effect picks up additional channels, such as lower competition for promotion and/or more favourable distribution of inputs and styles to be produced. In any case, controlling for the inclusion of these additional channels does not change the magnitude of the effect of firing spatially close co-workers.

To see more clearly how spatial distance matters, in Table 2.4, we break down geographical proximity into Circles as previously defined, and we also try to pick any possible non-linearity in its effect.

Because it is the spatially close peers who seem to matter, in column 1 of Table 2.4, we focus on the peers fired in Circle 1. We also distinguish between peers from the same block and peers from other blocks. We find similar results as in column 1 of Table 2.3. It also confirms that even among the peers who were in the immediate neighbourhood of the survivors, only the operators who belonged to the same block as the survivor mattered in terms of the effect on survivors' post-firing productivity.

Table 2.4: Post-Firing Changes in Productivity - Breaking Down Proximity

	(1) P3 Prod.	(2) P3 Prod.	(3) P3 Prod.	(4) P3 Prod.
Standardized Period 2 Theta	0.867***	0.848***	0.861***	0.852***
	(0.0326)	(0.0413)	(0.0361)	(0.0355)
# Fired in Circle 1: Same Block	-0.0702*	-0.0558*	-0.0555	-0.0555
	(0.0376)	(0.0324)	(0.0379)	(0.0389)
# Fired in Circle 1: Other Blocks	0.0658			
	(0.0423)			
# Fired in Circle 2: Same Block			-0.0180	-0.0250
			(0.0265)	(0.0284)
# Fired in Circle 3: Same Block			-0.00281	-0.0109
			(0.0307)	(0.0328)
# Fired in Rest of the Block		-0.0268*	-0.0407	-0.0296
		(0.0150)	(0.0261)	(0.0278)
Constant	0.0802	0.315	0.288	-0.555
	(0.207)	(0.539)	(0.577)	(1.103)
Observations	294	294	281	265
Adj. R-Squared	0.757	0.762	0.762	0.758
No. of Op. Controls	YES	YES	YES	YES
Other Controls	NO	NO	NO	YES

Note: Productivity estimates on both left hand side (LHS) and right hand side are standardized. *No. of Op. Controls* implies that there were additional controls with the number of total operators in the pool from where operators were fired (e.g. Circle 1, Circle 2 etc.). Age (with quadratic specification), tenure and a dummy of whether supervisor was fired constitute the other controls whenever used. Standard errors are bootstrapped and clustered at block level. *, **, *** indicate significance at 10%, 5%, and 1% respectively. The results show that post-firing productivity drops more if a peer was fired from within an operator's own block and also from immediate vicinity than other locations.

We next focus on peers only within the same block. In column 2, we compare peers fired from Circle 1 to peers fired from rest of the block. Each peer fired from

Circle 1 is associated with a 0.06 standard-deviation fall in post-firing productivity, which is more than double the fall associated with a peer fired from rest of the block. In column 3 we break down rest of the block into concentric circles around a survivor but farther away from him. There does not seem to be any effect from any circle farther away than Circle 1, which is only marginally insignificant. The coefficient for rest of the block is also marginally insignificant, but becomes highly insignificant once we have other controls, such as age and tenure, in column 4.

To summarise, we find that the lay-off of peers from one's immediate neighbourhood is associated with a fall in survivors' post-firing productivity, and this finding is robust to alternative specifications. This fall is higher than the effect of the firing of a peer from any other location. Even within the group of immediate neighbours, only firing of operators who belong to the same block as a survivor matters in terms of the surviving worker's post-firing productivity.

Supported by our qualitative observations, we want to argue that this negative association between peers' firing and survivors' post-firing productivity arises from workers becoming upset by the management's decision to fire their close colleagues. These effects could not have been generated by unobserved structural changes or instability after the labour unrest, because these factors are unlikely to vary with spatial exposure to firing. Our hypothesis gain support from the fact that this association with falling productivity is stronger when the firing took place among peers belonging to the same block and who worked in the closest spatial proximity to one another.

However, at least three other alternative but possible stories can also produce the same results. It could have been the case that in the post-firing period management was punishing operators whose friends (i.e. neighbours) were more disruptive during the protests. If the punishment were proportional to the number of neighbours fired, and if this punishment were to lead to falls in productivity, this would also produce the results we have shown so far.

To investigate whether this could indeed be the case, we investigate evidence of management's attitude towards the survivors. To do so, we look at the allocation of styles - a decision made by the distributors under direct supervision of the FIC (precisely the person the workers had harassed during their protest). Different styles have different piece rates, some of them being potentially higher or lower compared to their complexity. This complexity would have been captured by the style fixed-effects τ_s that we had estimated in Equation 2.1. If the FIC or management wanted to punish certain operators, they could potentially do so by allocating the relatively worse styles (which have relatively lower piece rates compared to their complexity, and hence lower τ). The weighted sum of style fixed-effects ($\tau_s * \rho_{ims}$) for all the styles (call it *rent*) that the survivors worked on in the post-firing period should be lower for operators for whom any negatively biased allocation decisions were made.

Table 2.5: Alternative Explanations of Punishment and Lost Learning

	(1) P3 Rent	(2) P3 Rent	(3) P3 Prod.
Standardized Period 2 Theta	0.0593	0.0523	0.837***
	(0.0664)	(0.0734)	(0.0394)
# Fired in Circle 1: Same Block	0.0406	0.0419	-0.0454
	(0.0871)	(0.0871)	(0.0338)
# Fired in Block Except in Circle 1	0.0809**	0.0825**	-0.0182
	(0.0327)	(0.0373)	(0.0140)
Std. Theta of Fired Peer in Circle 1			0.0849*
			(0.0445)
Std. Theta of Fired Peer in block except Circle 1			0.0309
			(0.0426)
Constant	-0.896	-2.764	0.309
	(1.292)	(3.227)	(0.652)
Observations	294	277	294
Adj. R-Squared	0.0512	0.0514	0.765
No. of Op. Controls	YES	YES	YES
Other Controls	NO	YES	NO

Note: All LHS variables are standardized. *Theta of Fired Peer* refers to Period 1 productivity of fired peers, averaged over all the fired peers. *No. of Op. Controls* implies that there were additional controls with the number of total operators in the pool from where operators were fired (e.g. Circle 1 etc.). Age (with quadratic specification), tenure and a dummy of whether supervisor was fired constitute the other controls whenever used. Standard errors are bootstrapped and clustered at block level. *, **, *** indicate significance at 10%, 5%, and 1% respectively. Columns 1 and 2 show that there is no evidence of surviving peers being punished by the management, at least in terms of style allocation decisions. Column 3 shows that we find drop in post-firing productivity even controlling for productivity of fired peers.

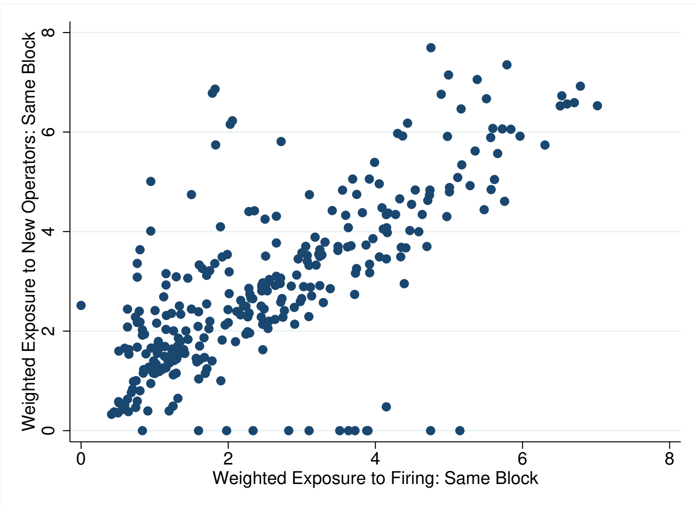
Hence, in columns 1 and 2 of Table 2.5 we regress the rent that the survivors gained during the post-firing period on the dummy variable of being fired. If any particular bias was targeted toward survivors who had more disruptive friends/neighbours, we would expect to see a negative correlation between the number of peer fired and the rent earned from styles. But in Table 2.5 we find no such correlation. If anything, it seems that the more peers fired, the more the rent that appears to be earned by the survivors. Thus, we can rule out the alternative story of punishment driving our results.

Another story that can fit our results is that workers suffer from the absence of particularly productive peers with whom they had positive and productivity-enhancing interactions. To check this we run the same specification as in column 2 of Table 2.4, but we additionally control for the average productivity of the fired peers. The results are reported in column 3 of Table 2.5. Although we lose statistical significance a little, the coefficient for Circle 1 falls by only about a fifth and the average productivity of fired peers seem to be positively correlated with post-firing productivity. On the other hand, if the productivity of fired peers dropped because of lack of productive interaction with the fired peer, we would expect that the productivity of fired peers would be negatively correlated with post-firing productivity of survivors.

Finally, it could also be the case that the post-firing productivity of survivors dropped because they were helping the newly hired operators who came to replace their fired peers. The greater the number of fired peers, the greater the number of newly hired workers, and so, survivors who lost more peers in the lay-offs would be spending more time helping new co-workers. This scenario could fit our results, and could show a drop in productivity with respect to fired peers.

So far, we have identified the negative impact of fired co-workers exclusively resting on cross-sectional data. Figure 2.3 shows that in the cross-section it will be extremely hard to disentangle the effect of fired workers from the effects of newly hired workers since the two are, by construction, very strongly correlated with each other.

Figure 2.3: Correlation between Exposure to Firing and Exposure to New Operators



Note: This figure plots the values of spatially weighted exposure to firing, computed from Equation 2.3 in the paper, with spatially weighted exposure to new operators, computed in the same way. It shows a high correlation between the two measures.

To overcome this challenge, we take advantage of the fact that new workers were hired to replace fired workers in two waves (in July 2014 and August 2014). We exploit the within-survivor time variation in exposure to new workers. We use standardised average wage earned per day for three post-firing months: June to August, 2014. Confining our sample only to survivors, we regress this wage on the number of old peers lost to firing in Circle 1, while also controlling for the number of new operators entering this circle in different months. The results are reported in Table 2.6.

Table 2.6: Alternative Story - Old Operators Help New Operators

	(1) Wage/Day (Survivors)	(2) Wage/Day (Survivors)	(3) Wage/Day (Survivors)
# Fired in Circle 1: Same Block	-0.0300	-0.0412*	-0.0432*
	(0.0222)	(0.0247)	(0.0246)
# of New Op in Circle 1		0.0209	
		(0.0231)	
# of New Op in their 1st month in Circle 1			-0.00628
			(0.0298)
# of New Op in their 2nd month in Circle 1			0.0527
			(0.0323)
Constant	0.309	0.361	0.409
	(0.507)	(0.504)	(0.506)
Observations	4,681	4,681	4,681
Adj. R Sq.	0.693	0.693	0.693
Individual FE	YES	YES	YES
Style FE	YES	YES	YES
Month FE	YES	YES	YES

Note: All LHS variables are standardized. Standard errors are clustered at individual level. *, **, *** indicate significance at 10%, 5%, and 1% respectively. Columns 2 and 3 show that we find negative effect of firing even when we control for new workers entering their neighbourhood, implying the negative association between firing and productivity is not driven by survivors helping new operators.

In column 1 of Table 2.6 we show a benchmark result in this specification to show the association between the number of fired peers and average daily wage. As before we find a negative association. In column 2, we add the control on number of new peers, which only strengthens our coefficient on exposure to firing. The coefficient on number of new operators itself is positive, although statistically insignificant. If the survivors were spending more time helping the new operators, we would expect the latter coefficient to be negative.

Finally in column 3, we break down this coefficient by separating the new operators into their corresponding waves: new operators who were in their first month into the factory (for July 2014, this wave would consist of only the workers hired in July 2014, and for August 2014, this wave would consist of only the workers hired in August 2014), and new operators who were in their second month (i.e. for August 2014, the operators hired in July 2014). We find that there is a very small negative association between survivors' productivity and new operators introduced into the circle, but this is only evident in the first month of their introduction. Again, this coefficient is not statistically significant, and it is one-seventh of the negative association between productivity and exposure to firing.

The above three tests confirm that the negative association we had found was not driven by at least the three alternative stories that could possibly fit the results. This lends support to our hypothesis that firing of peers left the survivors demotivated and/or upset, at least in the short run, and that this, in turn, led to the drop in their productivity following the firing period.

2.7 Conclusion

While understanding the dynamics of worker-management conflicts is important for all kinds of organisations, it is likely to be more so for those in developing countries. In recent years, firms in developing countries such as China and Bangladesh have been criticised for poor work-conditions, which in turn may lead to worker dissatisfaction and worker-management conflicts. These conflicts are likely to be resolved either by management and workers reaching mutual agreements, or by management taking stern actions. While poor work-conditions may have detrimental impacts on productivity in their own rights, worker-management conflicts and subsequent stern measures by management may lead to further worsening of mutual trust and cooperation, and in turn may have persistent impact on productivity as well.

In this paper we have presented evidences on how the firing of workers affects the co-workers who are left behind at the factory. Across a variety of empirical specifications, we find a drop in survivors' productivity that is larger for fired peers who used to work in their immediate vicinity. The evidence allows us to rule out several alternative but potential channels, and it suggests that the negative effect is likely driven by feelings of loss or anger towards the management, and by the possible perception among remaining workers that their workplace has become less enjoyable. This evidence is helpful in the context of understanding conflicts within an organisation, and how internal dynamics among the surviving workers are shaped in the wake of a severe step, in this case, the firing of disruptive workers following conflict.

Chapter 3

The Effect of Political and Labour Unrest on Productivity: Evidence from the Bangladeshi Garments Sector

Joint With: Rocco Macchiavello, Atonu Rabbani and Christopher Woodruff[1]

3.1 Introduction

Firms in low-income countries often face unstable external environments. Dealing with the uncertainty, a central task for entrepreneurs in any environment, is particularly challenging in unstable environments (McMillan and Woodruff [2002]). That entrepreneurs can adapt to deal with uncertainty is evident. The ready-made garment sector in Bangladesh, for example, has grown by more than 14 per cent per year between 1994 and 2014 despite an often-volatile business environment. But growth slowed to around 5 per cent in 2015, coincident with a marked increase in political strife in the country, and tragic accidents in the industry, including the Rana Plaza collapse, one of the world's worst textile industry accidents. Here, we focus on and isolate the role of political conflict on productivity in the sector. The political conflict is most visible through regular *hartals*, or general strikes called by

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political activists. The activists aim to shut down economic activity by preventing movement of people and goods, blocking roads and sometime destroying vehicles that venture out.

The popular view is that the hartals and blockades have a significant effect on the economy. A widely cited 2005 study by the United Nation Development Programme (UNDP) suggested that hartals cost the Bangladeshi economy 3 per cent to 4 per cent of GDP during the 1990s. A study by the Dhaka Chamber of Commerce and Industry (DCCI) put the cost to the Bangladeshi economy of political strikes at USD 200 million per day, with USD 45 million of the total coming from the garment sector.[2] The 22.5 per cent share of the costs the DCCI allocates to the garment sector is almost double the sector's share of the economy. The methods used to obtain these estimates are often unclear. The UN study, for example, starts by making the assumption that all output is lost on the day of a hartal, and that output is not made up on other days. This produces an estimated loss of 4.5 per cent of GDP, since 4.5 per cent of the days were days of hartals. This initial estimate is reduced to 3 per cent to 4 per cent for reasons that are not made entirely clear.[3]

The ready-made garment sector is one of the most important sectors in the Bangladeshi economy. It accounts for 80 per cent of Bangladesh's exports and around one-eighth of GDP. The sector has played a significant role in recent economic growth, which has lifted Bangladesh out of the group of low-income countries. Understanding how the sector has coped with hartals, and whether the costs are as large as many of the estimates, is therefore important.

In this paper, we use extremely detailed production data from 809 production lines in 33 large factories to examine the relationship between hartals and productivity in the garment sector. The factories are all located in Dhaka and the surrounding area. We have daily production line-level measures of productivity, absenteeism, and quality defects spanning the period from January 2012 through July 2014. Data collected from local news reports and other sources indicate that there were 96 hartal days involving 46 separate hartal events, 19 of which lasted for more than one day. We also collected data on labour protests, identifying 14 separate worker protests in the ready-made garment sector, four of which lasted for more than one day. Using an event-study approach, we examine how various production-related measures in the garment factories of our sample behave during the periods

[2]See http://archive.thedailystar.net/beta2/news/one-shutdown-means-tk-1600cr-losses/. Related estimates are discussed at http://archive.thedailystar.net/beta2/news/economy-reels-from-hartals/ and https://sourcingjournalonline.com/strikes-cost-bangladesh-3-billion-in-15-days/. (Accessed 5 November, 2015)

[3]See Sabet and Tazreen [2013] for a discussion of this and other commonly cited estimates of the costs of hartals.

of hartals and labour protests. The hartals are usually national in scope, but labour unrest is often local. For the latter, we use the factories in the areas in which the labour actions occurred.

We are not the first to examine the effects of hartals on output in Bangladesh, though our data allow a different and, in some dimensions, much more detailed analysis than the existing studies. The closest paper to ours is Ahsan and Iqbal [2014], which uses transaction-level trade data between 2005 and 2013 to examine the effects of hartals on garment exports from Bangladesh. They find that on the day of a hartal, exports fall by about 6 per cent, an effect which is partly offset during the week following the hartal. The effects are concentrated among smaller firms, and are stronger when the hartal extends for more than 12 hours.

We make two contributions relative to the literature measuring the effects of hartals. First, our factory-level data allow us to trace changes in productivity through several channels, including absenteeism, productivity conditional on the number of workers present, and supply-chains. Second, we benchmark the magnitude of the changes in productivity associated with hartals against the effects of another external factor affecting production: weather.[4] We find that the fall in productivity associated with sustained hartals is as large as the effect of a rise of about 7 degrees centigrade in average daily temperature. Similar fall in productivity associated with worker protests is as large as the effect of a rise of 7 to 10 degrees centigrade in average daily temperature..

This paper also adds to the literature on the effects of shocks on firms' performance and behaviour. For example, Adhvaryu et al. [2015] study the effects of pollution shocks on workers' efficiency in a sample of Indian garment manufacturers; Allcott et al. [2014] study the impact of electricity shortages; while Hjort [2013] and Ksoll et al. [2010] study the impact of a short-run spell of political instability in Kenya.

In addition to studies on the impact of specific shocks on firms' output and behaviour, a long-established tradition points at the negative impact of uncertainty on firms' investment decisions (see, for example, Bloom et al. [2007] and Bloom [2009]). Our analysis does not consider this additional channel.

The evidence in our paper are consistent with the possible effects of hartals being entirely driven by transportation and supply-chain effects. Factories adjust fully to shock on the first day of a hartal, with efficiency actually showing an increase

[4]Temperature has been shown to have a significant effect on output. Adhvaryu et al. [2014] use the conversion from fluorescent to LED lighting in a large garment factory in India to show that temperature on the production floor has a large effect on productivity. For a review of recent literature, see Dell et al. [2014].

on the day before and on the day of a hartal. However, the fall in output become larger when hartals last for several days. Ready-made garment worker protests are correlated with immediate fall in factory output which continues till the days immediately after the protest.

In what follows, we discuss the background of hartals and industrial unrest in Bangladesh in Section 3.2, and discuss the relevant data in Section 3.3. Section 3.4 presents the results. Section 3.5 concludes.

3.2 Background

With 4,000 to 5,000 factories employing roughly 4 million workers, Bangladesh is the second-largest exporter of ready-made garments in the world. These factories are mostly clustered in the areas around Dhaka or in Chittagong. Most of the fabric and other inputs used in production are imported, and, among the factories that export any part of their production, essentially all output is exported. The most important port for imports and exports is Chittagong. Contracts between factories and buyers usually call for goods to be delivered to the port in Chittagong by a certain date or, failing that, to be shipped by air at the factory's expense to London or some other destination. In our data, all factories are located around Dhaka; for these factories, the availability of transportation links to Chittagong, and, particularly, the one main two- and four-lane highway between Dhaka and Chittagong, is crucially important.

Bangladesh's political instability is viewed as an important hindrance for the development of the ready-made garment sector and for the country's economic progress at large.[5] Historically, political strikes or *hartals* in Bangladesh have been one influential form of political protests. They essentially have served as a non-cooperation movement to create pressure regarding specific issues on the incumbent government. These strikes are usually called by opposition political parties through public announcements, at least one day before a strike. The strikes aim to shut down economic activity and transportation. In theory, the strikes are meant to be voluntary acts on the part of participants; but, in reality, the opposition party activists try to enforce the strikes through picketing and by stopping vehicles that defy the strikes. Strikers and law enforcement agencies often clash during the picketing, and destruction of vehicles operating in defiance of the strike is also common. In some extreme cases, there have been fatalities. The intensity of violence was particularly

[5]See, for example, Hussain [2015]. A survey of foreign buyers conducted by McKinsey in 2011 revealed that half of them considered 'political and economic instability' to be a highly important issue. Nevertheless, this is only the fifth-most important issue listed by the buyers. (McKinsey-Company [2011], page 10.)

severe in 2013 when there were serious political disagreements on various issues regarding the then-forthcoming national elections and trials of opposition-party political leaders accused of war crimes during the country's liberation war of 1971. For example, over the period from January to mid-May, 2013, there were about 55 days of political unrest including 27 national strikes. Media reports suggest that a total of 143 lives were lost; more than 3,500 people were injured; and more than 1,500 vehicles were destroyed because of strike violence (Sabet and Tazreen [2013]). During the strikes, many businesses shut down, and the transportation system comes to a near halt, either because the populace supports or simply fears the consequences of the strike. Public-sector offices remain open as an act of defiance, and in recent years private-sector offices, and especially factories in various manufacturing industries, have remained open as well. However, lack of transport on the road implies that it is harder for workers and managers to travel to and from the offices and factories. In addition, transport of raw materials where needed, and physical movement of other materials or documents are difficult. Given the importance of the port of Chittagong to the ready-made garment sector, the effects of transport on the supply chain may be a particularly relevant channel. Several analysts have argued that the strikes create considerable economic loss for the country. Hussain et al. [2014] estimate the loss through forgone production in the 2013-14 fiscal year alone as $1.4 billion (Hussain et al. [2014]), with a further $2.2 billion lost in the first three months of 2015 (Hussain [2015]).

Worker unrest specific to the ready-made garment sector has also become more frequent in recent years. The reasons for actions by workers range from factory-specific issues, such as delays in salary or bonus payments, to industry-wide issues, such as minimum-wage rates. The lives of many workers in the sector were lost in well-publicized tragedies including the Tazreen Factory fire on November 24, 2012, and the Rana Plaza collapse on April 24, 2013. A typical protest in our context involves workers from a ready-made garment factory or factories leaving work and gathering on streets to protest against the factories or the industry management at large. The workers initiating the protest at one factory might also ask the workers at neighbouring factories to join. These protests can become violent, causing damage to the factories and leading to clashes between protesters and law enforcement agencies.[6] The effect of these protests is likely to be more severe for factories whose workers participate in the protests. These factories may shut down completely for a day or more. But there may be some effect on other nearby factories even when their workers do not participate. For instance, the conflicts may increase stress and

[6]See http://www.thedailystar.net/news-detail-231317 for an example.

create distractions for both workers and managers in other factories. These distractions might affect productivity of the workers, and divert the attention of managers away from regular operation management. Fearing that its workers may join the protests, nearby factories' managers may shorten work hours or reduce work load on the protest days in an attempt to appease their workers. Any of these channels may lead to a fall in productivity in the factories.

3.3 Data

We use data from three sources. First, we use detailed production data collected from 33 factories in and around Dhaka. The production data were gathered in the context of two previous projects that involved interventions providing supervisory training to either production-line workers or existing supervisors in factories in the sector. (See Macchiavello et al. [2014] for a description of the first project.) All of the factories produce woven or light knit products (e.g. t-shirts). The factories are not a random sample of factories in the sector, but are broadly representative of factories working for the middle to higher tier of foreign buyers. Table 3.1 provides some summary statistics for the factories, which are all suppliers of well-known European brands. They are large, with a typical factory having around 20 production lines and more than 1,000 employees. The available data were not uniform across all the factories. Of the factories we consider, 28 of the 33 had good data relevant for study of efficiency (referred to as *Sample: Efficiency* in Table 3.1), while 22 of them had good data relevant for study of absenteeism (referred to as *Sample: Absenteeism* in Table 3.1). The main tables on analysis make clear which sample we consider when studying a particular outcome variable. Figure 3.1 shows the size of the factories from which most of our data come, relative to the distribution of all of the factories in the buyer ACCORD.[7]

For all of the factories in the sample, we have continuous data over at least four months. We focus our analysis on the sewing sections, home to about two-thirds of the factory's workers. For our purposes, the most important measures are absenteeism, line runtime, quality defects, and efficiency. All of these measures are collected directly except for efficiency, which we calculate according to the industry practices, and which is essentially a measure of output per worker-hour. Output,

[7]The Accord on Fire and Building Safety in Bangladesh (ACCORD) is one of two agreements among brands that were forged following the Rana Plaza disaster. The program website describes the ACCORD as a 'legally binding agreement between brands and trade unions designed to work towards a safe and healthy Bangladeshi Ready-Made Garment Industry'. (http://bangladeshaccord.org/ - accessed 3 November, 2015)

Table 3.1: Key Descriptive Statistics for Production Data

	n	Mean	Std. Dev.	Min	Max
Total Number of Sample Factories	33				
- Sample: Efficiency	28				
- Sample: Absenteeism	22				
# of Lines in Factory in a day	12037	22.42	15.75	1	88
SAMPLE: Efficiency					
Standard Minute Value (SMV)	157,258	13.46	9.78	0	712
Runtime (Hrs)	170,008	10.44	2.71	0	24
Efficiency (%)	159,849	0.51	0.21	0	1.6
SAMPLE: Absenteeism					
Standard Minute Value (SMV)	106,841	14.90	12.26	0	712
Runtime (Hrs)	121,502	10.12	2.35	0	24
Absenteeism (%)	117,871	0.08	0.11	0	1
Efficiency (%)	105,707	0.54	0.29	0	1.6

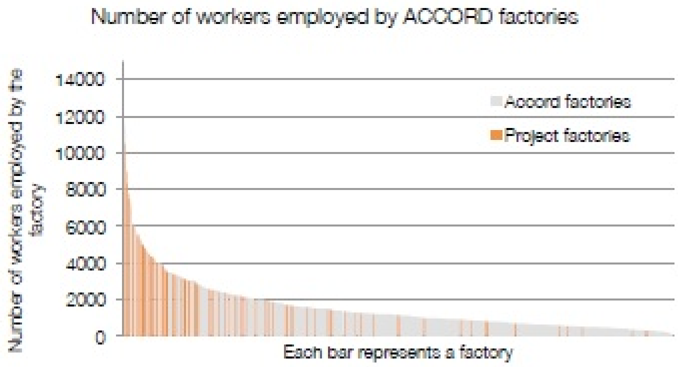
Note: Sample Period = January 2012 - July 2014; Total Factory-Days = 12,037; Total Line-Days = 269,833. The total number of 33 factories were classified into broadly two sub-samples called *Efficiency* and *Absenteeism*, based on the adequacy of relevant data useful for various analysis on efficiency and absenteeism, respectively. Some factories are included in both the sub-samples. In some of the tables reported in this paper, we sometimes also refer to a sub-sample called 'Phase 2', which were basically 20 factories from Phase 2 of the larger project. But all of these factories are contained within *Efficiency* and *Absenteeism* sub-samples.

however, is measured not in the number of products but in the number of minutes a fully efficient line would take to sew the output, calculated in a manner described below.

Second, we collect information on two types of external shocks faced by the producers. The first set of shocks are the political strikes, or hartals, that are usually called by the political opposition parties in Bangladesh in protest of certain acts or decisions by the government. The frequency of hartals increased markedly in the period 2012-2014, particularly in the lead up to national elections held in January 2014. During the period from January 2012 to May 2014, 96 instances of strikes directly affected Dhaka. We focus on these strikes since all our sample factories are located in Dhaka.[8] The distribution of political strikes across months is shown on Figure 3.2. A main tactic of the political strikes is to limit the movement of

[8] We do not record one additional hartal, which took place in Chittagong, as affecting the plants in our data. Chittagong is the main port through which inputs and output flow. Our results are robust to classifying this particular day as a hartal day.

Figure 3.1: Project Factories Compared with ACCORD Factories

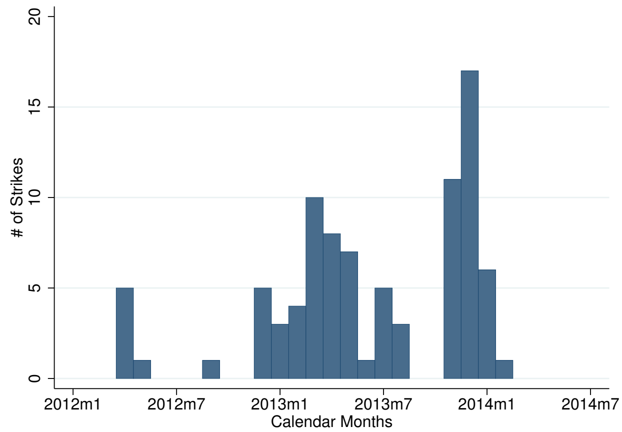


people and goods by closing roads to inflict economic damage and to raise visibility. Urban public transit shuts down during the strikes, and those traveling by car risk being assaulted, and having their cars damaged or even burned.[9] Businesses may or may not choose to shut down. The hartals are also marked by occasional violence between picketers and law enforcing agencies. We collected data on these strikes from The Daily Star, a widely circulated national daily newspaper. We also collected information about which political parties called for the strike, when the strike was called, and what the reasons for the strike were.

A review of the reasons the political parties cite for calling strikes indicates that they are largely associated with strictly political conflicts at the national level, rather than with any particular economic policy or any matter directly linked to the garment industry. Most strikes called by the major political parties in 2013 either stemmed from disagreement about how the national election of 2014 was to be conducted, or grievances over outcomes of legal cases filed against opposition leaders charged with war crimes during the 1971 war for independence. Although the causes of the strikes may be unrelated to production, they may affect production either by making it difficult for workers to reach factories or by limiting the transportation of goods, both inputs going to the factory and production going out. We posit that the first effect should be immediate, and the second more severe when the hartals last for several consecutive days. Because hartals are sometimes announced several days in advance, factories may anticipate these problems, and they may plan ahead to work around them. We leave it to the data to tell us how effectively they are able to address the challenges caused by the political strikes.

[9]The preferred mode of transport for foreign aid workers during hartals is an ambulance, hired in place of cars.

Figure 3.2: Intensity of Strikes in Different Months



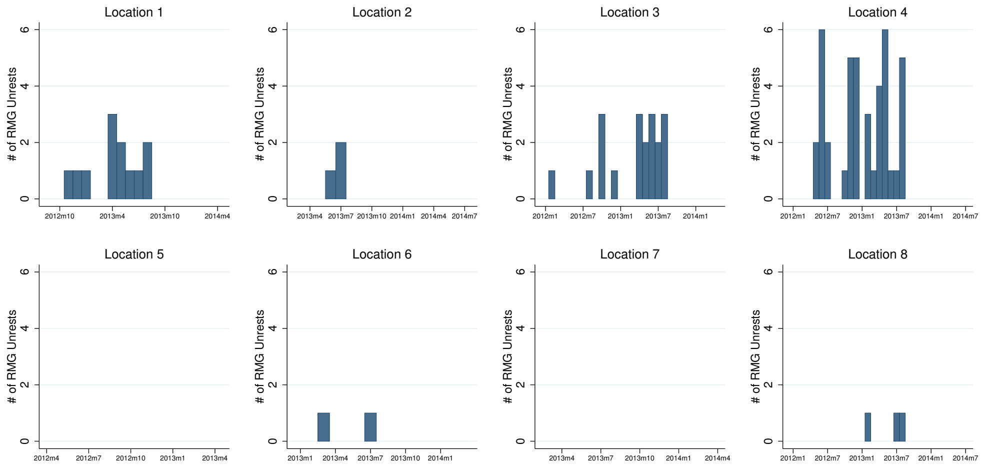
Note: The figure shows the intensity of hartals or political strikes in different calendar months of the sample period covered. Information on these strikes have been collected from a widely circulated national newspaper, The Daily Star. The peak of the strikes happened in November-December of 2013, immediately prior to a national election that was scheduled to be held in January 2014.

The second set of shocks we consider is more closely associated with the ready-made garment industry in Bangladesh. We collected data from The Daily Star accounts of ready-made garment worker protests, including the specific geographic areas involved in the protests. A typical protest starts at a single factory, with workers refusing to work and taking to the streets. These protests may involve all of the workers or only a fraction at a particular factory, but they are often joined by workers of neighbouring factories in the area. Figure 3.3 shows the distribution of the worker protests across time in eight different areas covered by our data. The duration of the protests can range from one day to more than one day. Conditional on successful negotiations, the workers return to the factories. Larger and more geographically spread protests have followed more serious incidents of factory fires and factory building collapse, including the Tazreen fire and the Rana Plaza collapse. Protests following these events also often last for a much longer period.

Finally, to benchmark the changes-in-production associated with the strikes against other shocks, we collect Bangladesh Meteorological Department data on temperature, humidity and rainfall from the Dhaka weather station. These data come at three-hour intervals, and we use simple averages of the temperature and humidity over either the 24-hour period, or the period from 6 a.m. to 6 p.m. on the production day.

Relationships of interest: We match the data on political strikes and ready-made garment worker protests to daily production data from the 33 factories in the

Figure 3.3: Intensity of RMG Unrests in Different Months (by locations)



Note: The above figure plots the intensity of RMG unrests in different calendar months of the sample period, shown with respect to locations of the factories in the sample. The locations have been defined as geographical clusters where the sample factories are located in. Information on these unrests have been collected from a widely circulated national newspaper, The Daily Star. There are 2, 5, 15, and 4 factories in Locations 1, 3, 4, and 8, while there is 1 factory in each of Locations 2, 5, 6, and 7. Location 1, 3, and 4 were mostly hit by RMG unrests in the sample period covered, while Location 5 and 7 did not have any such unrests.

greater Dhaka region. Production in the woven and light-knit sector is organized in production lines. The length of the line varies with the type of product being produced, with as few as 15 sewing machine operators (e.g., a line producing tank tops) or as many as 80 (e.g., a line producing pants). Each operator performs a specific task (e.g., sewing the side seam or the bottom hem). Production is sequential. Cut fabric enters at the start of the line and the completed product exits at the end of the line. Absenteeism can therefore be costly to efficiency; workers must move across or within lines to fill in gaps left by absent workers.

Each factory records data in its own way, and, often, the templates for recording data change across time within the same factory. The level of detail also varies across factories. We have harmonised the data so that they are comparable across factories, at least in terms of definitions. We are interested in several outcomes: First, we examine how strikes are correlated with the number of worker-hours in the factories. Worker-hours may change either because the number of employees who arrive for work changes, or because the start / end times are adjusted. We examine factory-level absenteeism rates and whether factories are more likely to shut production lines on days of political strikes or worker unrest. We also examine whether factories adjust to absenteeism, or expected absenteeism, by increasing the length of the work day. For most factories, we have line-level data on the number of hours the line operates.

Next, we examine whether the strikes are correlated with the efficiency with which lines operate and/or the quality of the output. By focusing on sewing, we are able to capture a measure of output which is very close to a pure quantity measure, at least within a given factory. Industrial engineers are trained to estimate the number of minutes a fully efficient worker would take to produce a given garment by considering each step of the sewing process and summing the time required for all individual steps. The times come from a combination of international databases and in-factory time-and-motion studies. The result is expressed as *standard minute values* - SMVs (or standard allowable minutes - SAMs). Multiplying the SMV by the number of units produced yields a measure of minutes of sewing output that is highly comparable across products. For example, if a line producing 1,000 shirts with an SMV of 15 minutes has production of 15,000 output minutes, a line producing 2,000 tank tops with an SMV of six minutes has production of 12,000 output minutes. Efficiency is then measured as the ratio of the output minutes and the amount of labour time - the sum of minutes worked by operators and helpers on the line over the same time period[10] - used to produce the output. The result, the industry standard measure of efficiency, is essentially an output-labour ratio:

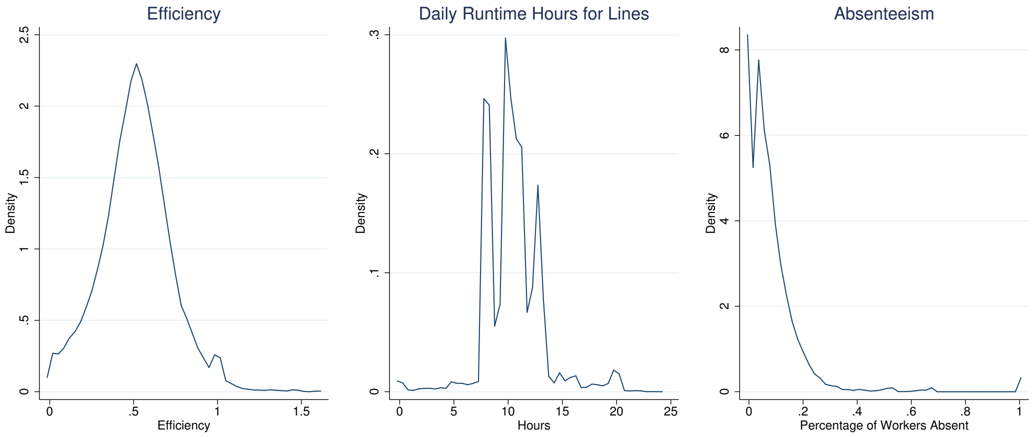
$$\mathit{Efficiency} = \frac{Output * SMV}{\{n(Operators) + n(Helpers)\} * \mathit{Runtime\ in\ Minutes}} \tag{3.1}$$

$n(Operators)$ and $n(Helpers)$ are the total number of operators and helpers, respectively, working in a given line. While, in theory, the SMV measure makes output comparable across factories, in practice, factories use different standards to assign the allowable time for each process. Some factories adhere quite closely to the international standards, allowing any local circumstances to be absorbed into the measured efficiency; others take the local circumstances into account in setting the SMV measure. However, careful analysis of the data leaves us confident that the measures are consistent within factories. All of our regressions will include either factory or line fixed effects to absorb any differences in the standards applied in measuring SMVs across factories.

Figure 3.4 shows the distribution of efficiency, line runtimes, and absenteeism for our sample of factories.

[10]We could improve this measure by a step if we had the wage bill for the line. However, the industry typically uses three grades for operators, and we most often know only the total number of operators, not the number by grade.

Figure 3.4: Distribution Plots



Note: The above figures plot the distribution of Efficiency, Daily Runtime Hours for lines and Rate of Absenteeism for workers. Efficiency for a given line in a given day is defined as the ratio of total time estimated to be required to complete whatever quantity of garments were completed and the actual time required to complete them. The former is estimated as the product of total outputs and Standard Minute Value (SMV). SMV is the average time required to complete one piece of a garment, as estimated by the factories for each style of garments. Daily Runtime Hours is the length of time (in hours) that a line operated for in a given day. Rate of absenteeism is the number of workers (operators + helpers) who were absent in a given line in a given day as a ratio of the total number of workers who were registered with that line. Most lines in the sample operate at a level close to 50%. Daily runtime has multi-modal distribution where most lines operate for 8, 10, or 12 hours in a day.

Quality defect rates are also of interest. Factories typically report both the number or percentage of garments that require some reworking, and the number or percentage that must be rejected. Reject rates are typically very low, averaging 0.6 per cent in our sample. Rework rates are much higher, averaging around 8 per cent (with a median of almost 6 per cent). Because the rework time is included in the measure of 'input minutes', the efficiency measure incorporates differences in quality.

Finally, we examine supply-chain issues with a measure of the inputs fed into the line on a given day. The input measure is the number of sets of cut fabric pieces entering the line. On the day of the strike, this should be affected only if the cutters are absent. But where strikes last for longer periods, the delivery of fabric and other inputs may be affected.

3.4 Results

We begin by examining how various outcome variables are correlated with the shocks, using the following equation.

$$Y_{ift} = \hat{\alpha} + \beta_1 H_t + \beta_2 R_{ft} + X_{it}^{'}\delta + \alpha_i + \gamma_m + \epsilon_{ift} \tag{3.2}$$

Y_{ift} is the outcome of interest for line i in factory f on day t. H_t is a dummy variable that takes the value one if day t was a strike day. Similarly, R_{ft} is another dummy variable that takes the value one if there was a ready-made garment industry protest in the neighbourhood of factory f on day t. $X_{it}^{'}$ is a set of time-varying controls for line i, α_i is line fixed effect, and γ_m is the year-month fixed effect.

Column 1 of Table 3.2 shows that incidences of political strikes, perhaps surprisingly, have a positive correlation with attendance; the absenteeism rate falls by just over 1 percentage point, against a sample mean of just over 8 per cent. However, absenteeism is measured at the line level, and is conditional on the line operating. Factories may adjust to worker absenteeism by moving workers from one line to another. This is required to prevent bottlenecks on lines, given the sequential nature of the production. If enough workers are absent, a factory may choose to close one production line, and use the workers present from that line to fill in the gaps on other lines. In column 2, we therefore examine whether production lines are more likely to be closed during hartals. Indeed, we find that political strikes are associated with a 0.9 per cent increase in the likelihood a line is closed. Together, then, the data on absenteeism per line and line closures suggest that there is almost no change in worker attendance during hartals. This likely reflects the fact that workers tend to live quite close to the factories, and, thus, they may not rely on buses or other transportation to travel to the factory.

Factories may also adjust the length of the work day, and column 3 shows they do so. However, the average time a line operates increases by only about four minutes (0.06 of an hour) on days with political strikes. The net effect of changes in absenteeism and runtime is an increase in the number of worker-hours on lines that operate (Column 4), which is offset by the reduction in the probability a line operates.

While labour input does not seem to change during the political strikes, it does seem to fall during ready-made garment worker protests. Both absenteeism and line closures increase when there are protests, with absenteeism increasing by 0.4 per cent, and the probability a line closes increasing by 1.7 per cent. The line runtime also decreases, by just over 25 minutes (0.43 hours). The net effect is a

Table 3.2: Impact of Shocks

	(1) Absenteeism	(2) Pr(Line Closure)	(3) Runtime	(4) Av. Hrs.	(5) Efficiency	(6) Defect Rate
Strike	-0.0103***	0.00827***	0.0581***	6.764***	0.00113	-9.04e-05
	(0.00105)	(0.00189)	(0.0146)	(1.006)	(0.00154)	(0.000456)
RMG Protest	0.00384**	0.0169***	-0.431***	-27.62***	-0.0168***	0.00211
	(0.00164)	(0.00319)	(0.0380)	(2.223)	(0.00310)	(0.00154)
Above Median SMV	0.00308*		0.341***	70.48***	-0.0158***	0.0122***
	(0.00170)		(0.0392)	(4.045)	(0.00412)	(0.00161)
Factory Weekend	0.00370	0.283***	-2.362***	-102.5***	-0.0165***	0.00222**
	(0.00246)	(0.00164)	(0.0811)	(4.732)	(0.00224)	(0.000958)
Constant	0.105***		10.65***	454.8***	0.454***	0.0634***
	(0.00559)		(0.0761)	(9.514)	(0.0133)	(0.00528)
Observations	87,477	184,928	150,969	150,530	148,298	84,326
Line FE	YES	YES	YES	YES	YES	YES
Year-Month FE	YES	YES	YES	YES	YES	YES
SE Cluster	LINE	LINE	LINE	LINE	LINE	LINE
Clusters	368	445	623	665	665	412
Sample	Absenteeism	Phase 2	Efficiency	Efficiency	Efficiency	Quality
Adj. R Sq.	0.214		0.526	0.792	0.196	0.563

Note: All but column 2 in the above table are OLS regressions with lines that are open in a given day. Columns 1 shows how *Absenteeism*, the share of workers absent for a given line in a given day, changes during the shocks. Column 2 reports marginal effects from corresponding Logit regression on the probability of a line being open during a Strike day, RMG Protest day or during the Factory's regular weekend day. Column 3 checks how *Runtime*, the total number of hours a line operated in a given day, changes during the shocks. Columns 4 reports results for *Available Hours* which is the total number of worker-hours used by a line-day. Column 5 reports regression for *Efficiency* (Total Input Minutes/SMV*Output), while Column 6 reports regression for *Defect Rate*, which is the share of total production from a line-day that had to be altered. This uses only the factories for which we had adequate quality data. Column 1 uses factories for which we have adequate absenteeism data. Column 2 uses factories only from Phase 2, while columns 3-5 use all factories used in Efficiency sample. *Strike*, *RMG Protest* and *Factory Weekend* are all binary variables which takes the value 1 for a given day if that day was a Strike day, RMG Protest day or an otherwise regular Factory Weekend, respectively. *Above Median SMV* is a binary variable that takes the value 1 if the garment that a given line of a given factory works on a given day has an SMV that is above the median SMV within the factory over the whole sample period. All standard errors are clustered at line level; the number of clusters (lines) are reported under *Clusters*. *, **, *** indicate significance at 10%, 5%, and 1% respectively.

decrease of almost 28 hours of labour input per line, about 7 per cent of the sample mean of 406 worker-hours per line-day.

Do the disruptions decrease efficiency, either through the disruption to labour or other channels? Columns 5 and 6 on Table 3.2 show how efficiency and defect rates change during political strikes and worker protests. The first takeaway from the table is that there is very little or no change in efficiency and defect rates during political strikes. However, efficiency drops significantly during ready-made garment industry worker protests, reducing the output per worker by 1.68 percentage points, almost 3.5 per cent of the sample mean. There is no significant change in defect rates during these protests.

In sum, there are no contemporaneous changes in worker absenteeism or productivity during political strikes. In contrast, during ready-made garment worker protests in the immediate neighbourhood of the factory, absenteeism goes up, work-days become shorter, and output per worker-hour falls. As we have noted, the tactic of political strikes is to shut down transportation. The lack of a contemporaneous effect likely stems from the fact that workers tend to live close to the factories. Apparently, a strike of a single day does not disrupt transportation enough to affect productivity at the factories. The hartals often extend for more than one day, however. We next turn to assessing the possibility that the longer hartals have cumulative effects that are more significant.

To understand cumulative effect of shocks, we estimate the following equation:

$$Y_{ift} = \alpha_i + \sum_k \beta_{1k} H_t^k + \sum_r \beta_{2r} R_{ft}^r + X_{it}^{'}\delta + \gamma_t + \epsilon_{ift} \tag{3.3}$$

where H_t^k is a dummy variable that takes the value one if day t was a strike day for k-th time in a row, and $k \in \{1, 2, 3, 4, 5 \textit{ and above}\}$. Similarly, R_t^r is a dummy variable that takes the value one if day t was a ready-made garment protest day for r-th time in a row and $r \in \{1, 2 \textit{ and above }\}$. The other variables are the same as Equation 3.2. There are only few ready-made garment protests lasting more than one day, which is why we limit the categories for persistent protest effects to all events lasting more than a single day. On the other hand, incidences of consecutive hartals are more common, allowing us to examine effects of continuous strikes in a more nuanced manner. Because many hartals are announced in advance, we consider anticipation effects below.

The results are presented on Table 3.3. Note that the variables are defined such that the variable for 1^{st} Strike is zero on the second consecutive hartal day, when the 2^{nd} Strike in a Row variable is set to one. This means that the coefficients for multiple hartal days are independent rather than additive. Looking first at labour input, we now see a clear positive changes in labour hours on the first day of a hartal. Absenteeism conditional on the line operating falls by 0.9 percentage points (column 1); the probability a line is closed falls by 2.5 percentage points (column 2); and runtime increases by about four minutes. Worker hours increase even conditional on a line operating; the actual change is larger because the number of lines operating also increases.

However, consecutive hartals result in a reversal of this pattern. Beginning on the second consecutive day, lines are around 4 percentage points more likely to be closed; runtime flips from positive to negative -though it is still fairly modest in magnitude - on the 4^{th} consecutive day; and total labour hours conditional on a line operating also flips on the 4^{th} consecutive day. Absenteeism remains lower than the

Table 3.3: Cumulative Effect of Shocks on Production

	(1) Absenteeism	(2) Prob(Line Closure)	(3) Runtime	(4) Av. Hrs.	(5) Efficiency	(6) Defect Rate	(7) Input
1st Strike	-0.00871***	-0.0248***	0.0624***	6.122***	0.00204	-5.17e-05	-16.92
	(0.00107)	(0.00229)	(0.0150)	(1.057)	(0.00154)	(0.000564)	(14.57)
2nd Strike in a Row	-0.0107***	0.0456***	0.146***	13.75***	0.00374	-0.000752	4.882
	(0.00116)	(0.00265)	(0.0221)	(1.393)	(0.00241)	(0.000801)	(19.45)
3rd Strike in a Row	-0.0129***	0.0338***	0.0661**	6.558***	0.00155	0.000919	-41.41**
	(0.00105)	(0.00277)	(0.0279)	(1.794)	(0.00299)	(0.000917)	(19.72)
4th Strike in a Row	-0.0193***	0.0100	-0.187***	-4.227*	-0.00676	-0.000241	-64.63**
	(0.00181)	(0.00625)	(0.0405)	(2.365)	(0.00442)	(0.000954)	(30.99)
5/6/7/8th Strike in a Row	-0.00378**	-0.0308***	-0.126**	-2.671	-0.0138*	-0.000215	-140.5***
	(0.00174)	(0.00893)	(0.0630)	(4.733)	(0.00727)	(0.00109)	(35.53)
1st RMG Protest	0.00394**	-0.00622*	-0.442***	-24.43***	-0.0150***	0.00329**	-211.8***
	(0.00189)	(0.00376)	(0.0421)	(2.263)	(0.00334)	(0.00150)	(25.77)
2/3/4th RMG Protest in a Row	0.00360	0.0644***	-0.410***	-34.64***	-0.0209***	-0.00278	-130.3***
	(0.00249)	(0.00432)	(0.0670)	(4.278)	(0.00529)	(0.00266)	(39.01)
Above Median SMV	0.00308*		0.341***	70.49***	-0.0158***	0.0122***	-307.5***
	(0.00170)		(0.0392)	(4.045)	(0.00413)	(0.00161)	(25.46)
Factory Weekend	0.00365	0.282***	-2.363***	-102.6***	-0.0167***	0.00222**	-233.8***
	(0.00246)	(0.00161)	(0.0811)	(4.727)	(0.00224)	(0.000962)	(23.59)
Constant	0.105***		10.65***	454.8***	0.454***	0.0634***	1,535***
	(0.00559)		(0.0761)	(9.512)	(0.0133)	(0.00528)	(79.60)
Observations	87,477	184,928	150,969	150,530	148,298	84,326	39,878
Line FE	YES	YES	YES	YES	YES	YES	YES
Year-Month FE	YES	YES	YES	YES	YES	YES	YES
Clusters	368	445	623	623	623	412	159
Sample	Absenteeism	Phase 2	Efficiency	Efficiency	Efficiency	Quality	Efficiency
SE Cluster	LINE	LINE	LINE	LINE	LINE	LINE	LINE
Adj. R Sq.	0.214		0.526	0.792	0.196	0.563	0.293

Note: All but column 2 are OLS regressions conditional on a line being open; column 2 reports marginal effects from corresponding Logit regression on the probability of a line being open during n-th Strike day, or n-th RMG Protest day or during the Factory's regular weekend day. *n-th Strike/RMG Protest in a Row* indicates there were n-1 strikes/RMG unrests in the immediate previous days. All of these variables are binary. In addition, *Above Median SMV* is a binary variable that takes the value 1 if the garment that a given line of a given factory works on a given day has an SMV that is above the median SMV within the factory over the whole sample period. All standard errors are clustered at line level; the number of clusters (lines) are reported under *Clusters*. *, **, *** indicate significance at 10%, 5%, and 1% respectively.

periods unaffected by hartals for all of the periods.

The pattern reversals that are evident with consecutive hartals in the labour input data also appear in the efficiency results. There are very small, statistically insignificant but precisely measured, changes in efficiency for when hartals last three days or less (column 5 of Table 3.3). But the change in efficiency is negative when the duration extends to four days, and significantly negative for duration beyond four days. Column 6 shows that there is no change in defect rates, regardless of the length of the political strike.

The changes in outcomes are much less stark for worker protests across different lengths of duration. The drop in labour input is slightly lower on the second or following days after the start of a protest, with the probability a line is closed showing the largest change. But the change in total labour input (total worker-hours per line) is always negative, and the change in efficiency is very similar as well. Defect rates increase slightly, but significantly, the first day of a protest, but return to normal levels the second day.

These results suggest that factories see little impact from a hartal lasting a single day, but begin to suffer more negative effects during hartals lasting more than a few days. Disruption of the supply chain is one possible channel for the negative effects. Road closures over a series of days may begin to disrupt both the ability to receive inputs, and the ability to ship outputs. Unfortunately, we do not have data on shipments into and out of the factory. But we do know the input fed into each line on each day. These inputs are measured as the number of sets of inputs required to produce one complete garment. Under normal condition, inputs will begin to enter the line a day or so before the line begins producing the style, and these inputs will flow at a steady rate, more or less matching the level of daily output. Disruptions in the supply chain will reduce the flow of inputs into the line.

The last column of Table 3.3 shows how the quantity of inputs at the line level changes because of political strikes and worker protests. We see that there is no change during hartals with a duration of less than three days, but the inputs begin to drop on the third consecutive day of political strikes. The drop becomes larger for hartals of longer duration. This pattern is consistent both with supply disruption from road blockades, and with the pattern we saw in the length of the work day and line efficiency, both of which turn negative after four consecutive days of hartals. In contrast, the change in inputs during ready-made garment worker protests is immediate. Inputs fall on the first day and remain lower for protests of longer duration. We posit that the likely cause of this is absenteeism in the cutting section rather than supply-chain disruptions.

Anticipation and recovery effects: Because many political strikes are announced in advance, factories may adjust production before the strikes in anticipation of disruptions during the strikes. Although we have seen that production is not affected by political strikes of a single day, it is not always clear in advance whether the strikes will extend beyond a day. After the strikes are over, we might also ask whether production is affected during a readjustment phase. To examine these effects, we run regressions with the follow specification:

$$\begin{aligned} Y_{ift} = \alpha_i + \beta_1 H_t + \sum_m \beta_{2m} H_t^m + \sum_m \beta_{3m} (H_t^m * H_t) + \beta_4 R_{ft} + \\ \sum_n \beta_{5n} R_{ft}^n + \sum_n \beta_{6n} (R_{ft}^n * R_t) + X_{it}^{'} \delta + \gamma_t + \epsilon_{ift} \quad (3.4) \end{aligned}$$

where $m \in$ {-3 and earlier, -2, 1, +1, +2, +3, +4 and after} indicates that day t is m

days before (negative signs) or after (positive signs) a political strike. The pre-strike indicator variable takes the value one only if the strike is announced by that date. Since ready-made garment worker protests are not announced in advance, there are no anticipation effects for the protests. However, we do test for recovery effects after protests. Because the day after a political strike may also be a strike day, we also interact the pre- and post-strike dummies with a dummy indicating that the day itself is a strike day. Hence, if March 1 is a strike day and March 2 is not - and no further strikes are anticipated at that point - then the recovery will be reflected in the coefficient β_2 alone. If March 2 is also a strike day, then the dummy variable associated with β_3 and day $t+1$ will also turn on, and the effect will be the sum of β_2 and β_3 for day $t+1$.

The results are presented on Table 3.4. It is easiest to consider an isolated hartal that is anticipated at least three days in advance. We see that absenteeism falls before the hartal, significantly so one and three days before the hartal. Absenteeism continues to be reduced on the strike day and the day after, but then increases to higher-than-normal levels from the 4^{th} through the 7^{th} day after the strike. Line closures (column 2) accentuate the change in absenteeism, the day before and the day after the strike, when the probability a line is closed is significantly reduced, and in the period four to seven days after the strike, when the probability of a line closing increases along with absenteeism. Factories run production lines for slightly longer periods on the two days before and the day of the strike, and then run for shorter periods on the two days after the strike. Combined, the results in the first three columns indicate that labour inputs increase the day before and the day of the strike, and then return to normal levels or fall just below normal levels two to seven days after the strike. Efficiency (column 4) follows a roughly similar pattern: increasing the day before the strike, remaining abnormally high through the day after the strike, and then dropping below normal levels two or three days after the strike.

Summing the coefficients across the 11-day period suggests that neither labour inputs nor efficiency change during an isolated political strike lasting a single day. The probability a line is closed increases by about 1 percentage point over the 11 days, offset by a roughly 0.7 percentage point increase in the time a line is run conditional on operating. Changes in absenteeism and efficiency vary over the duration, but the net effect is nearly zero, suggesting that the hartal shifts production across days, but does not alter the net output. In contrast, there is evidence that worker protests have more enduring effects on productivity, with efficiency falling by slightly more than a single percentage point for several days after the protest.

Table 3.4: Breaking Down Cumulative Effect of Shocks on Production

	(1) Absenteeism	(2) Prob(Line Cls.)	(3) Runtime	(4) Efficiency
3+ Days Before a Strike Day	-0.0110***	0.0242***	-0.110***	-0.00304
	(0.00131)	(0.00250)	(0.0266)	(0.00320)
2nd Days Before a Strike Day	0.000228	0.0896***	0.0837***	-0.00330
	(0.00122)	(0.00267)	(0.0248)	(0.00303)
1st Day Before a Strike Day	-0.00675***	-0.0274***	0.193***	0.00961***
	(0.000981)	(0.00366)	(0.0237)	(0.00204)
Strike Day	-0.0135***	0.00546	0.174***	0.00502**
	(0.00180)	(0.00347)	(0.0218)	(0.00248)
1st Day After a Strike Day	-0.00568***	-0.0169***	-0.0667***	0.00353*
	(0.000892)	(0.00221)	(0.0189)	(0.00185)
2nd Day After a Strike Day	0.00497***	-0.0241***	-0.200***	-0.00534***
	(0.00120)	(0.00228)	(0.0237)	(0.00157)
3rd Day After a Strike Day	-0.00165	0.0194***	0.0172	-0.00476***
	(0.00109)	(0.00206)	(0.0208)	(0.00173)
4/5/6/7th Day After a Strike Day	0.0113***	0.0145***	0.142***	-0.000755
	(0.00107)	(0.00163)	(0.0196)	(0.00178)
(Strike Day) * (3+ Days Before a Strike Day)	0.0192***	-0.0850***	0.226***	0.00419
	(0.00171)	(0.00630)	(0.0422)	(0.00460)
(Strike Day) * (2nd Days Before a Strike Day)	-0.00109	-0.0152***	-0.0155	0.00489
	(0.00171)	(0.00489)	(0.0475)	(0.00450)
(Strike Day) * (1st Day Before a Strike Day)	0.00477***	0.0559***	-0.451***	-0.0203***
	(0.00149)	(0.00490)	(0.0370)	(0.00363)
(Strike Day) * (1st Day After a Strike Day)	0.00321***	0.0922***	0.0855***	-0.00569**
	(0.00120)	(0.00389)	(0.0277)	(0.00284)
(Strike Day) * (2nd Day After a Strike Day)	-0.0106***	0.0422***	0.0347	-0.000613
	(0.00172)	(0.00429)	(0.0332)	(0.00260)
(Strike Day) * (3rd Day After a Strike Day)	0.00100	-0.0840***	-0.0573*	0.00431
	(0.00133)	(0.00644)	(0.0337)	(0.00306)
(Strike Day) * (4/5/6/7th Day After a Strike Day)	0.00881***	-0.0823***	-0.0642**	0.00104
	(0.00164)	(0.00405)	(0.0266)	(0.00316)
RMG Protest Day	0.00391**	-0.00880**	-0.484***	-0.0169***
	(0.00194)	(0.00377)	(0.0444)	(0.00364)
1/2/3rd Day After a RMG Protest Day	-0.00207	0.0137***	-0.132***	-0.0111***
	(0.00173)	(0.00406)	(0.0313)	(0.00304)
(RMG Protest Day)*(1/2/3rd Day After a RMG Protest Day)	0.00528	0.0607***	0.248***	0.00808
	(0.00326)	(0.00657)	(0.0753)	(0.00615)
Constant	0.103***		10.64***	0.454***
	(0.00560)		(0.0763)	(0.0133)
Observations	87,477	184,928	150,969	148,298
Clusters	368	445	623	623
Sample	Absenteeism	Phase 2	Efficiency	Efficiency
Adj. R Sq.	0.219		0.527	0.196

Note: All Specifications include (a) SMV Factory Weekend Covariates (b) Line FE (c) Year-Month FE. Days before a strike are days only after the corresponding strike was announced - i.e. when the upcoming strike was anticipated. All standard errors are clustered at Line Level. Factories used for all the specifications are from *Efficiency* Sample. *, **, *** indicate significance at 10%, 5%, and 1% respectively.

Benchmarking the effects: The changes in output during worker protests is substantial, but there is almost no change in output during political strikes unless they continue for several consecutive days. But how substantial are these changes? One way to benchmark them is to compare the magnitudes against changes in output caused by other factors outside the firm. Adhvaryu et al. [2014] find that temperature has a substantial effect on productivity, measured as the output-to-target ratio, in a large Indian garment manufacturer. We measure the effect of temperature and humidity on efficiency to provide a benchmark for the magnitude of the political and labour disruptions. The weather analysis also serves to check the veracity of the production data as a whole by providing a comparison with results from outside Bangladesh.

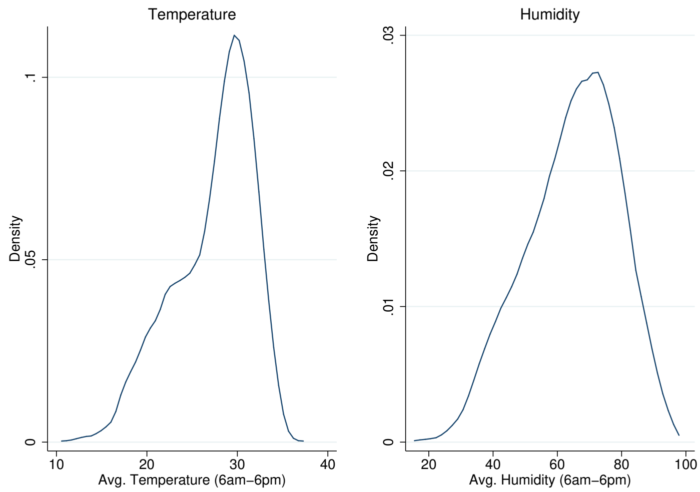
The Dhaka Meteorological Station measures temperature and humidity at three-hour intervals. Our output data have a daily frequency. We average the five temperature and humidity readings between 6 a.m. and 6 p.m. on each date. The distribution of these average readings is shown on Figure 3.5. We see that most of the temperatures are between 18 and 33 degrees centigrade, and most of the humidity measurements are between 35 and 90 per cent. Column 1 of Table 3.5 shows that both temperature and humidity have highly significant effects on productive efficiency. An increase in temperature of one degree decreases productive efficiency by 0.21 percentage points, not quite half a per cent of the mean efficiency level. We note that this is very close to the magnitude of the effect Adhvaryu et al. [2014] find (0.23) in a linear specification in their Indian factories. Humidity has a somewhat smaller effect, decreasing efficiency by 0.04 percentage points for each percentage point increase in humidity.[11]

We might expect temperature to have highly non-linear effects, with an increase in one degree having a larger effect in the upper range than the lower range. However, Figure 3.6 shows that this is not the case. The figure graphs coefficients from a regression using dummies for each degree, allowing us to trace out the effects by degree. The effect is surprisingly linear. Note that in this regard, our results differ from Adhvaryu et al., as they find a much large effect as temperatures rise from around 20 to 24 degrees, and again from 30 to 34. The second and third columns of Table 3.5 add additional controls, which have only a very small effect on the temperature measures.

The results on Table 3.3 indicated that an instance of continuous hartals is correlated with a reduction in productive efficiency by just under 1.4 percentage

[11]The standard deviation of humidity (14.3) is more than three times that of temperature (4.4), so the effect of a standard deviation change in temperature is almost double the effect of a standard deviation change in humidity.

Figure 3.5: Daily Temperature and Humidity over Sample Period



Note: Temperature and Humidity measures have been averaged over 3-hourly temperature readings for Dhaka city, collected from Bangladesh Meteorological Department.

points after a duration of five to eight days. There is a fall in efficiency by 1.5 to 2.1 percentage points during worker protests. The fall in efficiency during hartal is equivalent to a fall in efficiency because of an increase in temperature of about 7 degrees centigrade; the fall in efficiency because of protest is equivalent to that because of an increase in temperature of 7 degrees to 10 degrees centigrade.

3.5 Conclusions

Political strikes have been increasingly common in Bangladesh in recent years. There is considerable interest in and considerable differences of opinion about the effects of the strikes on economic activity. The ready-made garment sector provides an excellent place to examine these effects, as the sector accounts for roughly one-eighth of the country's GDP. Using very detailed production data from 33 large factories, we find suggestive evidence that factories adjust production fully in the face of a single, isolated hartal. However, there are more substantial negative changes in some outcome variables during both extended hartals and worker protests. Perhaps surprisingly, worker absenteeism does not appear to change much even during extended hartals. But we begin to see significant fall in inputs delivered to production lines after three days of strikes, and significant drop in efficiency after five days of hartals. This drop in efficiency because of longer-term hartals is similar to the effects of an

Table 3.5: Effect of Temperature & Humidity on Efficiency

	(1) Efficiency	(2) Efficiency	(3) Efficiency
Avg. Temperature (6am-6pm)	-0.00212***	-0.00204***	-0.00205***
	(0.000370)	(0.000379)	(0.000454)
Avg. Humidity (6am-6pm)	-0.000466***	-0.000413***	-0.000492***
	(6.80e-05)	(6.88e-05)	(0.000103)
Above Median SMV		-0.0158***	-0.0278
		(0.00412)	(0.0241)
Factory Weekend		-0.0164***	-0.0164***
		(0.00224)	(0.00223)
Ab. Med. SMV * Avg. Temp. (6am-6pm)			2.46e-05
			(0.000655)
Ab. Med. SMV * Avg. Hum. (6am-6pm)			0.000174
			(0.000162)
Constant	0.538***	0.521***	0.526***
	(0.0171)	(0.0174)	(0.0209)
Observations	159,849	148,298	148,298
Line FE	YES	YES	YES
Year-Month FE	YES	YES	YES
SE Cluster	LINE	LINE	LINE
Clusters	665	665	665
Sample	Efficiency	Efficiency	Efficiency
Adj. R Sq.	0.280	0.196	0.196

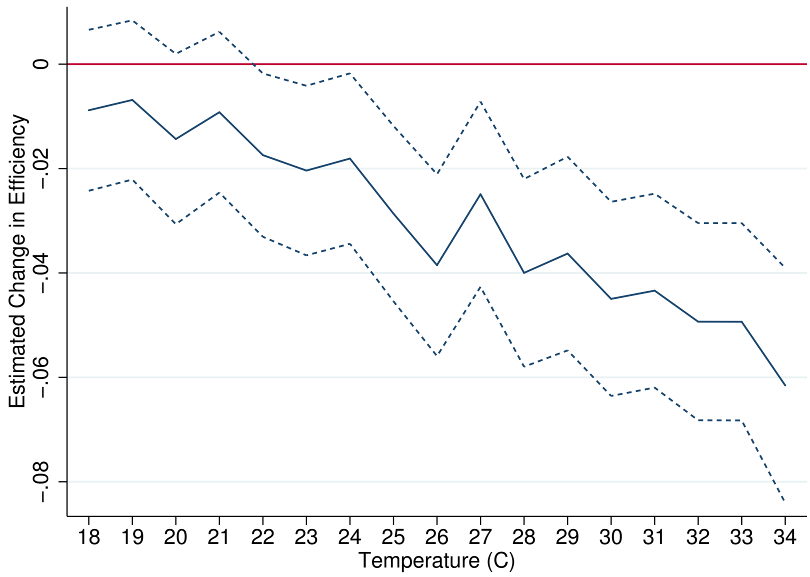
Note: Temperature and Humidity measures have been averaged over 3-hourly readings for Dhaka city, collected from Bangladesh Meteorological Department. *Above Median SMV* is a binary variable that takes the value 1 if the garment that a given line of a given factory works on a given day has an SMV that is above the median SMV within the factory over the whole sample period. *Factory Weekend* is a binary variable to indicate an otherwise weekend day for a given factory. All standard errors are clustered at line level; the number of clusters (lines) are reported under *Clusters*. *, **, *** indicate significance at 10%, 5%, and 1% respectively.

increase in temperature of seven degrees centigrade.

Worker protests are associated with larger and more immediate effects, and higher costs for the affected factories. During a protest, even one of a single day's duration, in the neighbourhood of a factory there is a fall in both the total number of labour hours and the output per labour hour. Moreover, the data suggest that the protests have lingering effects.

We are not aware of other estimates of the effect of ready-made garment worker protests on productivity in the sector. But our results on political strikes, and particularly those for an isolated hartal, although based on correlations, contrast with the estimates of more substantial costs from several sources. Methodologically, our approach is closest to work by Ahsan and Iqbal [2014], and our results appear to

Figure 3.6: Non-Linear Estimates for Impact of Temperature on Efficiency



Note: The above figure plots the coefficients from a regression of efficiency on dummies of average daily temperature (6am-6pm) and average daily humidity (6am-6pm). Omitted category is average daily temperature of 17C. 95% confidence interval reported by dotted lines. Factories used in this regression are from *Efficiency* sample. Temperature measures have been averaged over 3-hourly temperature readings for Dhaka city, collected from Bangladesh Meteorological Department.

be similar. They find modest effects of hartals on large firms, though their analysis does not differentiate between one-off and sustained hartals. Combining these two types of events, our data also suggest modest effects on our set of large ready-made garment factories.

Volatility in the external environment is a reality that firms in most low-income countries face. The data here suggest that firms have adapted to deal with these disruptions quite well.

However, at least one characteristic of this setting appears to be important. The fact that workers tend to live fairly close to the factories where they work may explain the lack of an effect on absenteeism and production on the day of the hartal. The effects of transportation operate through supply chains rather than through labour, and these effects show up only after several days of continuous strike activities.

The analysis suggests that the direct costs of hartals are negligible for all but hartals spanning many consecutive days. But there may be other channels not addressed by our data through which hartals affect production in the sector. For example, we are not able to say anything about costs to factories from delayed outgoing shipments. Contracts with buyers typically call for buyers to pay shipment costs from the port of Chittagong, but sellers are liable for the much more expensive air freight charges when goods are not delivered to the port on time. Hence, transportation delays that we believe affect inputs may also result in substantial costs to factories due to delayed outgoing shipments.[12] Finally, the hartals may have more substantial effects in other sectors of the economy, for example the retail sector, and on smaller businesses.

But, the data do suggest that estimates that rely on the assumption of an across-the-board proportional decrease in output across all sectors as a measure of the effect of hartals are likely to overstate the costs. Many firms and sectors are able to move production across time (some more easily than others), significantly attenuating the effects of hartals on output.

[12] A Bangladesh Garment Manufacturers and Exporters Association (BGMEA) report in April 2013 stated that 15 factories surveyed incurred additional air freight costs of $1.71 million as a result of hartals. (Dhaka Mirror: http://www.dhakamirror.com/business/15-rmg-units-incur-3-31m-loss-for-political-turmoil/ . Accessed 4 November, 2015.)

Appendix A

(For Ch. 1)

A.1 Proof of Theoretical Observations

A.1.1 Observation 1

Let e^*_{io} represent the equilibrium level of effort exerted by worker i in $t = 0$, which therefore solves Equation 1.3. In other words, for worker i in $t = 0$, e^*_{io} sets to zero the following net marginal utility of effort:

$$\frac{\partial U(e^*_{i0}, .)}{\partial e_{i0}} = \int \Big[W_1(e^*_{io} + \epsilon_i) + \frac{n-1}{n} H_1(z^*_{i0})\Big] g(\epsilon_i) d\epsilon_i - C_1(e^*_{i0}, \alpha_i) = 0 \qquad \text{(A.1)}$$

where, $z^*_{it} = e^*_{io} + \epsilon_i - \frac{1}{n}\sum_j e^*_{j,t} + \delta_{i0}$. I suppress the time subscript in ϵ_{it} since the i.i.d. values for ϵ are drawn from the same distribution of ϵ in each period. Although the realization for ϵ_{it} might vary across time, while computing expectation over all possible values of ϵ, the time subscript becomes redundant. Also, there is no ϵ_{-i} in z^*_{it} since $E(\epsilon_{it}) = 0$ by assumption.

Once the true ranks are revealed in $t = 1$, if worker i keeps his effort at e^*_{io} and takes everyone else's effort as given at $e^*_{-i,o}$, the net marginal utility evaluated at e^*_{io} is:

$$\frac{\partial U(e^*_{i0}, .)}{\partial e_{i1}} = \int \Big[W_1(e^*_{io} + \epsilon_i) + \frac{n-1}{n} H_1(z^*_{i0} - \delta_{io})\Big] g(\epsilon_i) d\epsilon_i - C_1(e^*_{i0}, \alpha_i) \qquad \text{(A.2)}$$

Notice that $H_1(.)$ is now evaluated at $(z^*_{i0} - \delta_{io})$ which is the revised perceived rank for $t = 1$ at e^*_{io}. Letting Δ be the difference of Equations A.2 and A.1, and using first order Taylor expansion we have:

$$\Delta_i = -\delta_{i0}\frac{n-1}{n}\int H_{11}(z^*_{i0})g(\epsilon_i)d\epsilon_i \tag{A.3}$$

Therefore, in $t = 1$, holding everyone else's effort fixed at $e^*_{-i,0}$ worker i will have the incentive to deviate from his effort from e^*_{i0} if $\Delta_i \neq 0$. But whether he increases or decreases his effort in $t = 1$ depends on the sign of Δ_i. If $\Delta_i > 0$, the marginal benefit at effort level e^*_{i0} in $t = 1$ outweighs the marginal cost of effort at e^*_{i0}. Because of the assumption on interior solution $(E[W_{11}(.) + (\frac{n-1}{n})^2 H_{11}(.)] < C_{11}(.))$ this difference between marginal benefit and marginal cost diminishes as effort goes up, in $t = 1$ worker i will increase his effort from e^*_{i0}. Conversely, if $\Delta_i < 0$, in $t = 1$ he will decrease his effort from e^*_{i0}. But whether Δ_i is positive or negative depends on the signs of both δ_{i0} and $H_{11}(.)$. I examine these cases below.

Case 1: $H_{11}(.) > 0$

Under the case where $H_{11} > 0$, the value of the integral in Equation A.3 is positive. Then the sign of Δ_i is determined entirely by the sign of δ_{i0}. If worker i underestimated his rank in $t = 0$, that is $\delta_{i0} < 0$, $\Delta_i > 0$ and hence worker i increases his effort in $t = 1$ relative to $t = 0$. Conversely, if the worker overestimated his rank in $t = 0$, that is $\delta_{i0} < 0$, $\Delta_i > 0$ and hence worker i decreases his effort in $t = 1$ relative to $t = 0$.

Case 1: $H_{11}(.) < 0$

Under the case where $H_{11} < 0$, the value of the integral in Equation A.3 is negative. Now, if worker i underestimated his rank in $t = 0$, that is $\delta_{i0} < 0$, $\Delta_i < 0$ and hence worker i decreases his effort in $t = 1$ relative to $t = 0$. Conversely, if the worker overestimated his rank in $t = 0$, that is $\delta_{i0} > 0$, $\Delta_i > 0$ and hence worker i increases his effort in $t = 1$ relative to $t = 0$.

It is also easy to see that when $\delta_{i0} = 0$, irrespective of the sign of $H_{11}(.)$, $\Delta_i = 0$. Hence, worker i exerts the same level of effort in $t = 1$ that he does in $t = 0$.

Proof of the above propositions relies on the assumption that a given worker takes everyone else's effort constant, and thus his equilibrium response is solely determined from his first order conditions. To the extent that his equilibrium response considers how other workers might change their behaviour (and hence feedback into $\tilde{e}_{-i,t}$ in his equilibrium response) the proof is an oversimplification.

However, it is indeed more likely that a worker's equilibrium response would hold $\tilde{e}_{-i,t}$ constant. Notice that when we start with e^*_{io}, it already considers an equilibrium response from other workers in period $t = 0$, however that equilibrium is determined. In period $t = 1$, to solve for the equilibrium completely, a worker would need to know $\delta_{-i,0} > 0$ of everyone else so that he can compute e^*_{-i1} and substitute this into his own first order condition. Additionally, he will also need to know α_{-i}. But both of these are private information. So, at least in the first month of treatment, he would not be able to know them. In the subsequent months, he could try to infer $\delta_{-i,0} > 0$ if he had known how others' ranks change, but the scope to learn is limited since worker i observes only his own rank. Any variation in his own rank would be caused by ϵ_{it} as well as $e_{-i,t}$, and $\epsilon_{-i,t}$. So only from his own rank it would be impossible to deduce what $e_{-i,t}$ is, especially when the number of peers is very large. Thus the best worker i can do is to assume everyone else's effort constant.

Alternatively, worker i can form an expectation of $\delta_{-i,0}$ and α_{-i}, and thus solve for equilibrium effort. But this would not only make the model significantly complex, and will also make the predictions vulnerable to what i's expectations about those parameters are, or how such expectations are formed.

A.1.2 Observation 2

In the instance when the firm chooses to rank workers privately, the results are similar as before when workers were driven by only self-image concerns. Therefore, in $t = 1$ the first order condition for a privately ranked worker is given by:

$$\int \Big[W_1(\tilde{e}_{it}) + \frac{n-1}{n} H_1(z_{i1})\Big] g(\epsilon_{it}) d\epsilon_{it} - C_1(e_{it}, \alpha_i) = 0 \tag{A.4}$$

where, $z_{i1} = z_{i0} - \delta_{i0}$. Let e^*_{pvt} solve the above equation for a privately ranked worker in $t = 1$.

In the instance when the firm chooses to rank workers publicly, in $t = 1$ the first order condition is given by:

$$\int \Big[W_1(\tilde{e}_{it}) + \frac{n-1}{n}(1 + s_i) H_1(z_{i1}) - M_1(\tilde{e}_{i,t-1} - \tilde{e}^f_{i,t-1})\Big] g(\epsilon_{it}) d\epsilon_{it} - C_1(e_{it}, \alpha_i) = 0 \tag{A.5}$$

Let e^*_{pub} solve the above equation for a publicly ranked worker in $t = 1$.

Ceteris peribus, the difference between LHS of Equation A.5 and LHS of Equation A.4 is the difference in marginal incentives between Public and Private ranking. It is given by the following:

$$\hat{\Delta} = \int \Big[\frac{n-1}{n} s_i H_1(z_{i1}) - M_1(\tilde{e}_{i,t-1} - \tilde{e}^f_{i,t-1})\Big] g(\epsilon_{it}) d\epsilon_{it} \tag{A.6}$$

The first part of RHS in Equation A.6 is the change in social-status utility from one additional unit of effort, while the second part is the disutility of outperforming friends from one additional unit of effort.

Let $x = \tilde{e}_{i,t-1} - \tilde{e}^{f}_{i,t-1}$. Recall that $H_1(.) > 0$ and $M_1(x) = 0$ for $x \leq 0$ by assumption. Also, assumption for interior solution states that $\frac{\partial^2 M(.)}{\partial e_{it}^2} > s_i \frac{\partial^2 H(.)}{\partial e_{it}^2}$, which translates to $M_{11}(.) > \left(\frac{n-1}{n}\right)^2 s_i H_{11}(.)$. Hence, at e^*_{pvt}, by the first two assumptions, $\hat{\Delta} > 0$ for $x \leq 0$. Because of the third assumption which implies that $\hat{\Delta}$ will fall with increase in effort, a publicly ranked worker will exert effort higher than e^*_{pvt}. In other words, when a publicly ranked worker is not ranked higher than his friends in the previous period ($x \leq 0$), he exerts more effort than a privately ranked worker because of social-status return on his effort ($e^*_{pub} > e^*_{pvt}$).

To understand what happens when x increases from $x = 0$, first note that:

$$\frac{\partial \hat{\Delta}}{\partial x} = \int -M_{11}(.) g(\epsilon_{it}) d\epsilon_{it} < 0$$

The last inequality follows from the assumption that $M_{11}(.) > 0$. In other words, comparing across workers when all of them are ranked publicly, $e^*_{pub,i} > e^*_{pub,j}$ when $x_i < x_j$.

Therefore, since $\hat{\Delta}$ is continuously decreasing in x, there exists a value $\tilde{x} > 0$ such that, $e^*_{pub} < e^*_{pvt}$ if $x > \tilde{x}$.

A.2 Style Rank Calculation

Ranks for each month were computed in the following three steps. In the first step, for each operator i of treatment group $g \in \{Private, Public\}$, and for each style and size $s \in S^g_i$ where, S^g_i is the set of all style-sizes that i worked on in a given month, his style-rank t^g_{is} is computed by comparing the average time he took to complete one set of sweater parts for style s to that of others in group g who also worked on s. $t^g_{is} \in I$ and $1 \geq t_{is^g \leq n^g_s}$ where n^g_s is the number of operators in group g working on style s, and a higher numerical value for t^g_{is} indicates a worse performance. The total time computed for each job includes the full working hours of a day when an operator was absent without prior notice, but excludes any days he had taken a prior leave for, or days when he is grated a medical leave. Styles where less than three workers (from the same treatment group) worked on were excluded.

In the second step, a weighted average of normalized style-ranks T_i is computed from all the style-sizes that i worked on. Ignoring the superscript for group, it is given by:

$$T_i = \sum_{s \in S_i} \left[\left(\frac{t_{is}}{m_s} \right) \left(\frac{q_{is}}{q_i} \right) \right] \tag{A.7}$$

where, m_s is the lowest rank (highest numerical value) for style s, q_{is} is the number of sweater sets of style s that i produced, q_i is the total number of sweater sets that i produced. $\left(\frac{t_{is}}{m_s}\right)$ normalizes each style-rank with respect to the lowest rank in that style, and thus it becomes comparable across styles. On the other hand, $\left(\frac{q_{is}}{q_i}\right)$ weighs each of the normalized style-ranks with respect to the share of a given style in an operator's total production of a month. Thus, the rank puts more weights on styles and sizes for which an operator produced relatively more than those where he produced less. I ignore the cases where only one worker worked on a given style and size. In the final step, the set of final ranks for treatment group g is given by $\{R_1, R_2, R_3, , R_N\}^g$ where N is the size of group g and $R_i \leq R_j$ *iff* $T_i \leq T_j$ for $i, j \in \{1, 2, , N\}$.

A.3 Additional Tables

Table A.1: Correlation Between Expected Rank
True Ranks (Private Treatment)

	(1) Rank Feb'16	(2) Rank Mar'16	(3) Rank Jul'16
Expected Rank	0.327***		
	(0.101)		
Rank in Previous Month		0.681***	0.609***
		(0.0677)	(0.0725)
Constant	43.78***	17.17***	21.12***
	(5.336)	(4.548)	(4.729)
Observations	112	113	110
Adj. R-Sq.	0.0787	0.472	0.390

Note: Column 1 shows correlations between workers' expected ranks as reported during baseline, and the true ranks they received in their first treatment month, February 2016. Column 2 shows correlation between ranks in March 2016 and February 2016, while column 3 shows correlation between ranks in July 2016 and June 2016. All regressions are cross-section regressions based on Private Treatment workers only. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. The results show that the correlation between true rank and workers' expected rank is weak, while that between true ranks in consecutive months is high.

Table A.2: Dynamic Response to Ranks

	(1)	(2)	(3)
	Ln(Wage)	Ln(Wage)	Ln(Wage)
		Pos.Feed.	Neg.Feed.
Private * Post	-0.00354	0.0197	-0.0176
	(0.00889)	(0.0139)	(0.0126)
Private * Post * Positive Change in Rank (Prev. 2 Mths.)	0.00787	0.00705	0.00451
	(0.0138)	(0.0138)	(0.0177)
Public * Post	0.00155	0.0176	-0.00723
	(0.00758)	(0.0139)	(0.0105)
Public * Post * Positive Change in Rank (Prev. 2 Mths.)	-0.00871	-0.0165	-0.00444
	(0.00938)	(0.0106)	(0.0149)
Post * Positive Change in Rank (Prev. 2 Mths.)	0.00185	-0.00133	0.00453
	(0.00856)	(0.00966)	(0.0135)
Observations	13,876	5,631	8,071
N(Worker)	366	146	216
Constant	YES	YES	YES
FE: Worker, Year-Month, Style	YES	YES	YES
Other Controls: Block Size	YES	YES	YES

Note: *1(Positive Change in Rank (Prev. 2 Mths.))* refers to a dummy variable that takes the value 1 if a worker observes his rank improve in the previous two months. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. The results do not provide any evidence of dynamic response to ranks.

Table A.3: Knowing Others' Ranks

	(1) Ln(Wage/Day) Public	(2) Ln(Wage/Day) Public	(3) Ln(Wage/Day) Private
Treatment * Post	0.00604	0.0233	0.0118
	(0.0106)	(0.0172)	(0.0127)
Treatment * Post * (# Peers Unexpectedly Ranked Lower in Prev. Mth.)	-0.0130***	-0.0106**	-0.00682
	(0.00497)	(0.00493)	(0.00751)
Treatment * Post * (# Peers Unexpectedly Ranked Higher in Prev. Mth.)	0.000524	-0.00347	-0.00381
	(0.00454)	(0.00547)	(0.00479)
Treatment * Post * 1[Rank$_{t-1}$ > Median of Friends in Block]		-0.0352**	
		(0.0167)	
Post * (# Peers Unexpectedly Ranked Lower in Prev. Mth.)	0.0114***	0.00977**	0.0113***
	(0.00433)	(0.00423)	(0.00401)
Post * (# Peers Unexpectedly Ranked Higher in Prev. Mth.)	-0.00365	-0.000574	-0.00300
	(0.00322)	(0.00395)	(0.00316)
Post * 1[Rank$_{t-1}$ > Median of Friends in Block]		0.0213**	
		(0.00909)	
Observations	9,702	9,361	9,437
N(Worker)	249	249	242
Constant	YES	YES	YES
FE: Worker, Year-Month, Style	YES	YES	YES
Other Controls: Block Size	YES	YES	YES

Note: *(# Peers Unexpectedly Ranked Lower in Prev. Mth.)* refers to number of peers from same block who a worker thought ranked higher than him during baseline survey but got ranked lower than him in previous month's rank during treatment period. Similarly for *(# Peers Unexpectedly Ranked Higher in Prev. Mth.)*. Standard errors are clustered at both worker and month level. *, **, *** indicate statistical significance at 10%, 5% and 1% significance levels respectively. The table shows that conformity behavior among Public Treatment workers were not driven by workers updating knowledge about their peers' relative ranks.

Appendix B

(For Ch. 2)

B.1 Additional Figures & Tables

Figure B.1: An Operator Working on a Manual Knitting Machine

Figure B.2: Knitted Sweater Part

Figure B.3: Timeline of Key Events in the Manual Knitting Section

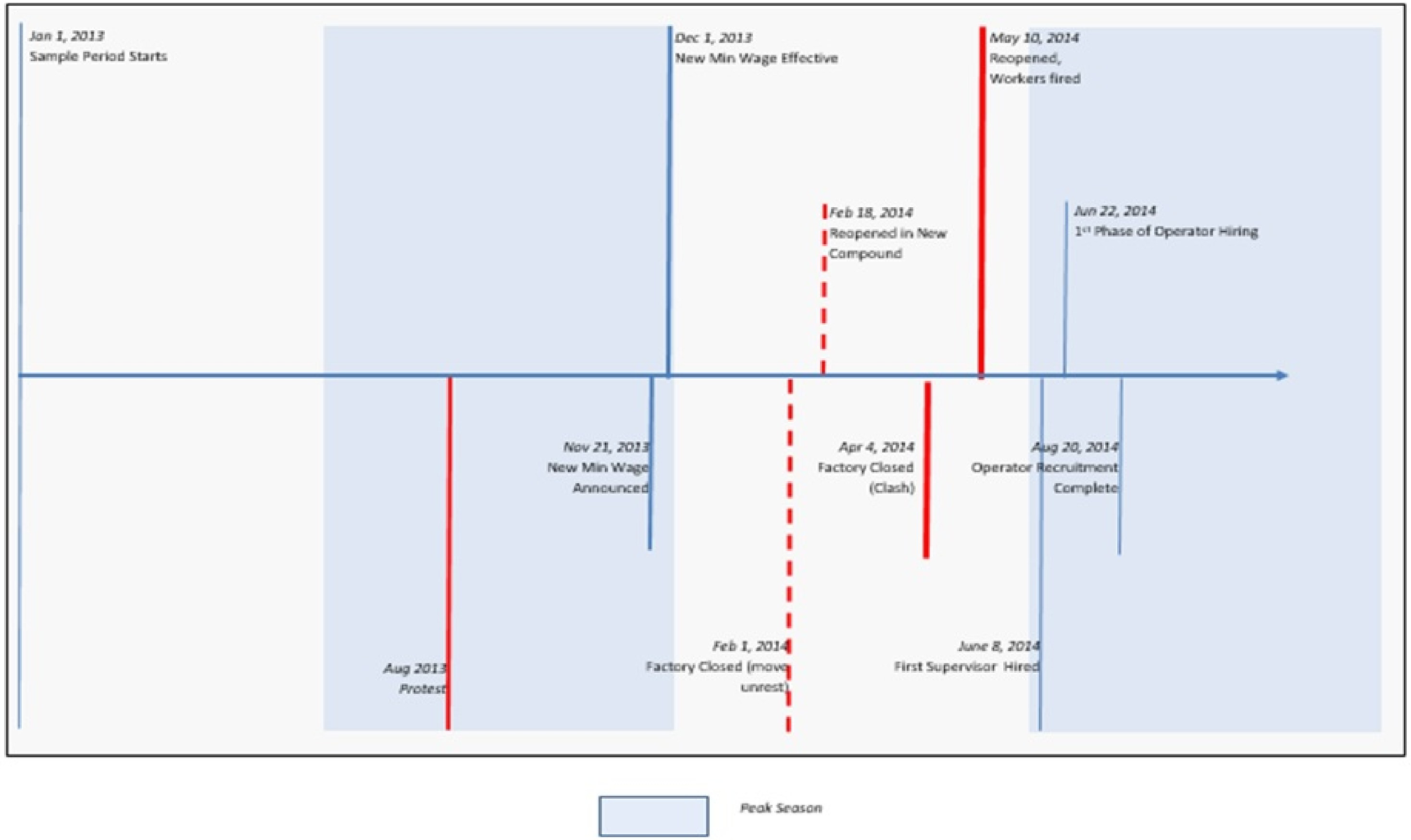


Figure B.4: Floor Chart of Manual Knitting Section immediately before Firing

Figure B.5: Organogram of Manual Knitting Section

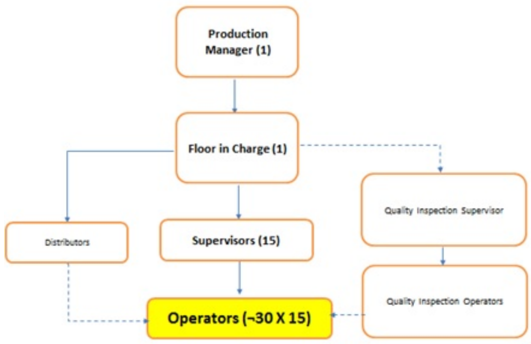
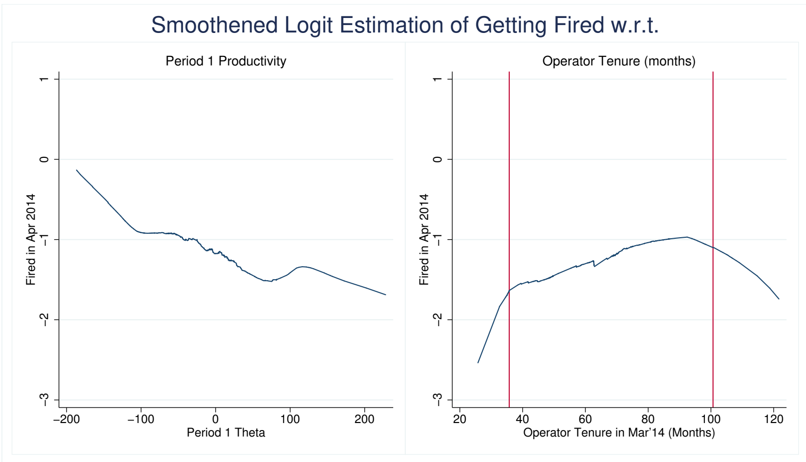


Figure B.6: Local Linear Regressions for Probability of Getting Fired



Note: The left panel plots a locally weighted logit regression of probability of getting fired with respect to Period 1 productivity. We indeed find a consistent negative correlation between the two - higher the productivity lower is the likelihood of getting fired. The right panel plots the same but against operator tenure (in months) in March 2014. We find a slight positive correlation between tenure and probability of getting fired.

Table B.1: Floor Observation 1

Time	Description of Activity
5:27-5:29 pm	Upon finishing his first back part, subject goes to the supervisor for measurement. Supervisor suggested using increased weight
5:37 PM	Chitchat with the operator to his left
5:42-5:43 pm	Upon finishing the second back part goes to the Supervisor for measurement once again. Comes back singing.
6:00-6:03 pm	After finishing the third back part subject notices a fault. The operator facing him (subject 2) also notices it and the two examine the piece. They fold it; hold onto the two ends, and together, stretches the piece
6:30 PM	Goes to the distribution room to get a cone of elastic thread and comes back in 30 seconds.
6:35 PM	Turns on the fan. The subjects machine is stationed just beside the wall where the switchboard is. Another operator playfully complains. Some chitchat among operators.
7:07 PM	Bundles the 24 sleeves he's finished, cleans up his machine and leaves floor.

Table B.2: Floor Observation 2

Time	Description of Activity
5:09 PM	Subject not in his station
5:30-5:56 PM	Subject arrives at his station and starts setting up his machine for a new style. A lot of non-work related chatting going on with the operator facing him.
6:00 PM	Operator another machine comes to the subject's station and borrows his operation breakdown.
6:12 PM	The operator to the subject's left comes to his station and helps him setup the machine. He gives hands on instruction for approximately 45 seconds.
6:16 PM	More small talk with the operators to his left and front. Subject is still setting up his machine.
6:17 PM	Subject finds that he forgot to change a part in the machine while setting it up for the new style that requires a different gauge. He tells that to the operator in front of him and starts changing it.
6:20- 6:27 PM	Subject fetches the supervisor to his machine. They talk about the technical stuff while the supervisor tries to tune the machine.
6:54 PM	Conversation with an operator to his front. Talks about the trouble he's having with his machine.
6:58 PM	Adjustments done and working with the machine starts.
7:00-7:01 PM	Takes a small sample of cloth he made to the supervisors, comes back in 30 seconds and compares his work with that of the operator to his left who is also doing a neck part.
7:07 PM	Cleans up and leaves the floor for the day.

Table B.3: Probability of Getting Fired with respect to Workers' Characteristics

VARIABLES	(1) Logit ME Prob(Fired)	(2) OLS Prob(Fired)	(3) OLS Prob(Fired)	(4) OLS Prob(Fired)	(5) OLS Prob(Fired)
Period 1 Theta	-0.000604*	-0.000601*	-0.000111	-0.0140**	-0.0101*
	(0.000362)	(0.000314)	(0.000323)	(0.00686)	(0.00551)
Ln(Operator Tenure)	0.139**	0.140*	0.127*	0.185***	0.162**
	(0.0665)	(0.0720)	(0.0685)	(0.0615)	(0.0587)
(Period 1 Theta)*(Operator Tenure)				0.00178*	0.00131*
				(0.000910)	(0.000740)
Constant		-0.849	-0.728	-1.186**	-0.996**
		(0.550)	(0.519)	(0.470)	(0.445)
Observations	388	388	388	388	388
R-squared		0.018	0.096	0.025	0.100
Block FE	NO	NO	YES	NO	YES

Note: Standard errors are bootstrapped and clustered at block level for models 1, 2, 4. Simple clustered standard errors (at block level) for models 3 5. Operator Tenure is in days calculated as the difference between an operator's joining date and the first day of March 2014. Change in Productivity in Period 2 is computed as ratio of Period 2 productivity to Period 1 productivity. The first column reports Marginal Effects from a logit regression with the same underlying specification. ***, ** and * denote p-values less than 0.01, 0.05 and 0.1 respectively. The results show the probability of getting fired is negatively correlated with previous productivity while positively correlated with the tenure of operators at the factory.

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